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# ASTRAL GRAVITATION

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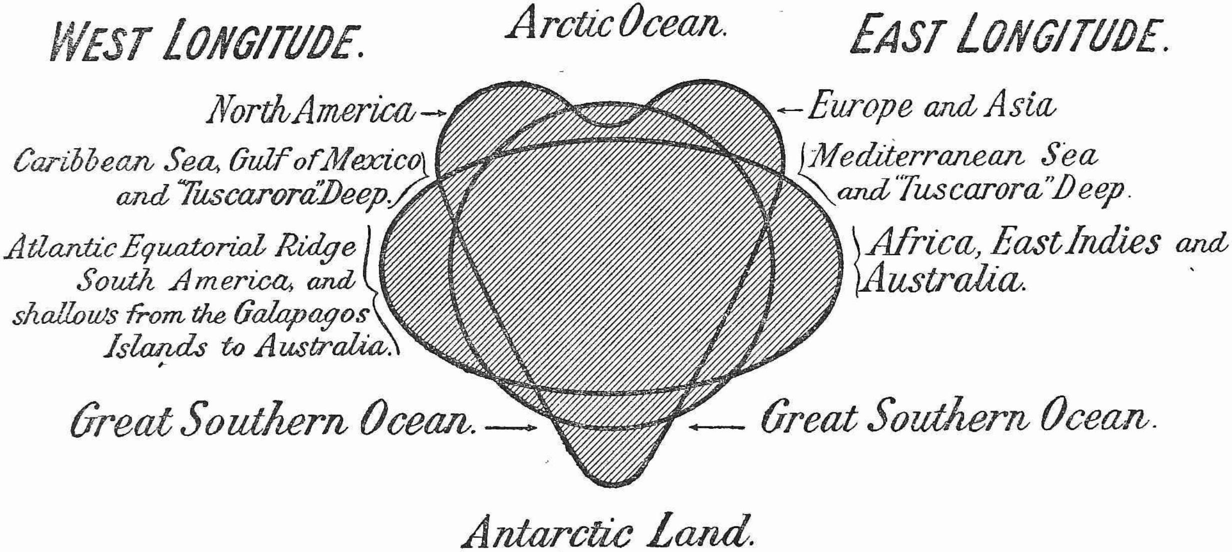


THE CARDIOID EARTH. See page 72, all the Third Essay, and page 179.

# ESSAYS

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BY

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# PREFACE

I HAVE endeavoured to make each of the following essays such as to give to any general reader a clear conception of the questions at issue in connection with the respective subjects, but have given references to works in which they are treated more in detail for those who may desire fuller explanations.

The chemical essay treats of what is certainly the most fundamental question in physical philosophy—namely, the question as to whether material atoms are indestructible or subject to natural transmutations; but those on 'Vis Inertiæ 'and Momentum,' on 'Ocean Currents,' and on 'The Spinning-Top' directly bear on and explain the action of astral gravitation, which is the question of most immediate practical importance in natural philosophy.

The views advocated in these essays are a natural development of the Newtonian laws of gravitation.

In connection with the revolving force of gravitation, I have to correct, or, I should rather say, to make accurate, the ratio of its action at different distances given in my hitherto published works. As that action depends on the excess by which the force of the nearer exceeds that of the remoter half of any rotating sphere, and as I found this, when (in the year 1870 or 1869) I calculated it for the orbits of the planets, to be approximately as the cubes of their distances from the sun,[1] I inferred that, to be quite accurate, this ratio must be calculated from the centre of gravity of the nearer hemisphere (the precise position of which we have no means of ascertaining). I chanced, however, recently, when dealing with a quite different question, to notice that the above ratio is accurate if calculated from the centre of the rotating sphere, and that I have, therefore, been mistaken in supposing it to be necessary to find the position of the centre of gravity of a hemisphere before being able to give, for the revolving force, calculations as mathematically accurate as those for the direct force. Granting that there is mathematical accuracy in treating the *direct* force as inversely as the *square* of the distance from the centre of the sphere, there is equal accuracy in treating the *revolving* force as inversely as the *cube* of the distance from that same point.

In a review of Todhunter's 'History of the 'Mathematical Theories of Attraction and the

[1] *The Ocean*, p. 82, 2nd edition.

'Figure of the Earth,' Lord Rayleigh says Maclaurin proved conclusively that the oblate spheroid form gives conditions of equilibrium if the Earth be considered as a fluid mass rotating;[1] and I presume that mathematicians are in agreement on that point. I have not sufficient knowledge of mathematics to form an opinion regarding it, and am content to accept their conclusions.

It is, however, one thing to prove that a certain shape would give equilibrium, and quite another thing to prove that the Earth could under the conditions treated by Maclaurin get precisely the shape so required. The latter appears the more important question in natural philosophy; and, without the abstruse mathematics required by Maclaurin for his purpose, I have, in Proposition XXVII., book x., of 'The Ocean,' 2nd edition, given a simple mathematical demonstration to the effect that a fluid Earth, or an ocean covering the Earth, could not attain a form of equilibrium under the supposed conditions. The rotation of the Earth incessantly thwarts an incessant effort of gravitation to create that equilibrium. I have myself used the term 'equilibrium' in the above Proposition, but not in the sense intended by Newton and Maclaurin. They suppose an equilibrium in which the fluid can rest, but I have shown that the same circulation which changes the form from a sphere

[1] *Scientific Papers by John William Strutt, Baron Rayleigh*, i. 198 (1899). Republished from *The Academy* 1874, p. 176.

to an oblate spheroid is continuously necessary for the maintenance of the oblate spheroid. Whilst the change is in progress there is an excess of motion in one direction which becomes equalised; but the form then attained cannot be more than a close approximation to the oblate spheroid which Maclaurin shows to give mathematical equilibrium, presuming not only his mathematics but also the data on which they are based to be correct.

Todhunter says (p. 196, vol. i.): 'We can show 'clearly that certain conditions must hold for equili- 'brium; but it is not quite obvious that if these 'conditions are satisfied there will be equilibrium.'

Whilst the Glossary at the end of this book has been with the printers, the strange absence of argument, or explanation, on Rankine's part in taking the accelerative force of gravity as the gravitation equivalent of energy, as shown at the foot of page 188, led me to think that he might perhaps have adopted it in some matter-of-course manner from Joule's mechanical equivalent of heat. Joule's law, roughly stated, is to the effect that the quantity of heat requisite to raise the temperature of one pound of water one degree Fahrenheit, is equivalent to a quantity of mechanical force sufficient to lift a weight of 772 pounds one foot, or to lift one pound 772 feet. Though I do not find Rankine's equivalent to have been expressly intended to be based on Joule's law, the erroneousness of his adoption of 32·2 foot-pounds as the gravitation equivalent of energy is made clearly apparent by Lord Kelvin's statement that Joule's equivalent of heat is '772 times the amount of 'work required to overcome a force equal to the 'weight of one pound through a space of one 'foot.'[1] The argument I have given in connection with diagrams 32 and 33 shows that the force which lifts the one pound weight 772 feet is 1,544 times greater than the force of gravity, or weight, which causes it to fall one foot; because the force which lifts one pound a height of 16·1 feet in one second is equivalent to twice the force of gravity which constitutes the weight of one pound. The error is not in the statement of Joule's law, nor in Lord Kelvin's explanation given above, provided it be recognised that the gravity equivalent of force is the space through which gravity moves a body, not that through which it is moved against gravity; so that the force which lifts one pound 16·1 feet is two gravity pounds of 16·1 poundals each, not one pound of 32·2 poundals. Lord Kelvin's statement given above, which was made about the time when Rankine's mistake originated, seems to show that the error made by Rankine consists in his having treated the space traversed against gravity as its measure, and having then applied to the *facts* stated

[1] *On the Origin and Transformations of Motive Power.* Lecture by Professor William Thomson, now Lord Kelvin. *Royal Institution Proceedings*, 1856, p. 201.

by Joule and Lord Kelvin the measure of one gravity pound, consisting of the 32·2 poundals, which represent the accelerative force of gravity, and also truly represent the poundals of work done against gravity, but constitute a force equivalent to a weight of two pounds. At any rate, whatever may be the manner in which the error has been made, it intrinsically consists (as shown in the Glossary, pages 184 to 189) in the fact that the *two* gravity pounds requisite to lift a pound weight 16·1 feet have virtually been treated in gravitation measure as *one* gravity pound, though the latter term has not, as far as I know, been made use of. Neither the term nor its true quantity seems to have obtained recognition. A true pound of force is, in fact, a gravity pound, whereas the pound of gravitation measure is two pounds of force.

Though I do not find that Joule expressly says so, he seems to have taken it for granted that because the force with which a pound weight strikes the ground when dropped from a height of 772 feet is equivalent to the force requisite to project it to a height of 772 feet, the falling force is therefore equivalent to the lifting force; and he would therefore see no reason to object to Rankine's equivalent of energy. I venture with confidence to say that, as a question of pure theory, granting that forces are as the quantities of motion they cause, 772 feet may be taken as the measure of the force which causes the weight to fall that distance. The force which has to project the weight upwards is not acting on the weight whilst it is falling, but has to do its work against the already existing force which causes the 772 feet of downward motion. If that downward force were removed, then the same quantity of force would carry the weight 772 feet upwards, but, as there is no such removal, double the quantity of work has to be done, and double the force is requisite to do it. Though this latter view is, I say, intrinsically the doctrine of Newton's Second and Third Laws of Motion, Joule's idea to the effect that the two apparent motions of 772 feet are effects of equivalent forces seems to be due to statements made by Newton, to which I have alluded on page 183, Art. *Astral Gravitation*, to the effect that opposing forces 'destroy' each other; but I have repeatedly argued to the effect that those casual statements are at variance with his own laws and demonstrations. Newton was at times drifted into such statements by the error in his First Law of Motion, which obliged him to ignore the transverse action of gravitation because it duplicates motion given by that law; and it obliged him to ignore the action of astral gravitation because it stops motion given by his First Law.

The pendulum serves to illustrate the above argument. Let the ball of the pendulum be one pound weight. That weight represents the force which causes it to fall 16·1 feet in one second; and, forces being as the quantities of motion they cause in any unit of time, the force measured in feet is 16·1 foot-pounds of force. At the end of the second the velocity is no longer at the mean rate of 16·1 feet per second, but is at the rate of 32·2 feet per second, which is equivalent to two pounds of force, and by the restraint of the rod it is directed upwards against the existing one pound of downward force, which does another 16·1 feet of downward motion in the next second whilst the upward force does 32·2 feet of work upwards; the resulting lift of 16·1 feet being the difference between the upward action of two pounds of force and the downward action of one pound of force. In the two seconds 64·4 poundals, or four pounds of work, are done vertically by the one pound weight, whether it be allowed to fall freely or be attached to the pendulum rod. The slight amount of friction at the point of suspension of a well-made pendulum *in vacuo*, would perhaps make the continuity of its motion differ almost imperceptibly from perpetual motion if gravitation did not resist the transverse motion. Nearly all the retardation of the pendulum *in vacuo* may, it seems to me, be considered due to the resistance of gravitation to the transverse motion, so that the impulse of a spring, or a weight, is required to maintain the transverse, and not the vertical motion. The work done by the spring to keep the pendulum going is in the earth's motion done by the transverse action of the sun's gravitation, which is the amount by which its gravitation exceeds its gravity, as explained in the Glossary. The force of gravity acts with the same freedom on a pendulum in vacuo as on the moon, but the transverse action of the earth's gravitation cannot have effective action on the pendulum proportionate to that which is effective in the moon's orbital motion, because the point from which it is suspended is fixed in relation to the earth's surface; and therefore in order to maintain the amplitude of the vibrations of a pendulum in vacuo, and thus perpetuate its motion, it seems to me that a force must be artificially supplied equivalent to the difference between the gravity and the gravitation of the earth added to the force of friction at the point of suspension.

To illustrate Joule's equivalent in accordance with the arguments of the annexed Glossary: 96 pounds of impressed force do 1,545·6 foot-pounds (or poundals) of work, and if the force be impressed vertically upwards against gravity (see diagram 33) will lift one pound 772·8 feet (approximately Joule's equivalent); because whilst the impressed force is doing 96 pounds of work, gravity does 48 pounds of work. Any portion of work which may be done by any impressed force against gravity, *either upwards or downwards*, is potential energy at the moment of the impressed force being expended, and then becomes effective in a rebounding or a heat-producing action of gravity. Every falling motion caused by gravity is a rebound from the action of impressed force as truly as the rebound of a spring on being released from the bending force.

Of course, as a measure, the pound of 32·2 poundals serves its purpose just as any arbitrary measure would do, provided all calculations take into account the fact that the force of gravity is really only half the quantity which the measure declares it to be; and that is, in practice, done by halving the true quantity of energy dealt with, though I do not find that those who halve the force in that manner have clearly recognised the reason why they are practically compelled to do so.

WM. LEIGHTON JORDAN.

25 JERMYN STREET, S.W
*August* 28, 1900.

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## *FIRST ESSAY*

## OCEAN CURRENTS

### PART I. INTRODUCTORY

RECENT discussions and opinions regarding the general course of the circulation of the ocean and the causes of the main features of that circulation make it expedient for me to offer some comments on the present position of the subject in such a manner that the arguments which are the basis of this essay may be independent of them; and, therefore, any reader who desires to obtain a general knowledge of the subject can omit this introductory part and consider the second part as complete in itself. It will be seen that one reason for this division is that there is some uncertainty as to the manner in which some of the 'Challenger' records of temperature should be treated, and I therefore refrain from allowing the main arguments of the essay to depend on such disputed data; but where the evidence of those records is undisputed I have freely used it in the second part.

In the year 1866 I submitted to the Council of the Royal Geographical Society a paper showing horizontal and vertical sections of a general system of oceanic circulation which, according to the arguments of that paper, the earth's axial rotation tends to cause by creating a conflicting action of gravitation; the earth's force tending to carry the water along with the earth's rotation, and all surrounding forces of gravitation tending to retard that motion. The Council, however, replied to the effect that the said sections showed that my theoretical views required the existence of a vertical circulation which had no actual existence. The main point was that I showed a conflicting action of gravitation to be constantly drawing the water, not only eastwards, but also *downwards*, throughout both of the temperate zones, and upwards at the poles and along the equator. It was urged that by such a circulation the great diversities of temperature in juxtaposition on the surface in the temperate zones would be carried down into the lower strata; whereas it was asserted to be well known that no such diversity existed in the under strata. It had long been known that fresh water is heavier at about 39° F. than at any other temperature; and it was supposed by most of the leading authorities on the subject that this *heavy* temperature pervaded all the deep parts of the ocean,[1] the colder surface water of the polar regions and the warmer surface water of the equatorial regions floating over the heavier water of the intermediate temperature. This opinion, which prevailed in 1866, ignored, on the one hand, observations which had shown the existence of much colder water in deep parts of the ocean, and on the other hand it also ignored the fact that experiments, more recent than the fresh-water experiments above alluded to, had shown that sea water has not a maximum density at 39° F., but continues to contract and increase in density down to its freezing-point.[2]

Investigations made by expeditions sent out by the Admiralty during the years 1868 to 1872 completely swept away the above idea regarding the distribution of temperature, as water north-west of the British Isles was found to decrease from about 50° F. on the surface to about 31° F. at the bottom of the ocean;[3] the water of the supposed heaviest temperature being found between what had been supposed to be the two opposite lighter temperatures. This confirmed the accuracy of older observations and experiments, to which sufficient importance had not been previously attached. It continued, however, to be insisted

[1] *The Depths of the Sea*, by C. Wyville Thomson, 2nd edition, 1874, p. 57.

[2] *Ibid.* pp. 302, 304, 307.

[3] *Ibid.* pp. 81, 310.

that the circulation could not carry streams of relatively warm water below colder water, as said to be required by my above-mentioned sections; and the 'Challenger' expedition was therefore sent out in 1872 provided only with thermometers which were not supposed to be able to register alternations of fall and rise of temperature in vertical planes corresponding to the alternations found in horizontal planes. The chief of the scientific staff of that expedition had, in 1872, given his opinion in the words: 'It may certainly 'be taken as the rule for all latitudes that, if we 'disregard the film which is affected by diurnal 'alterations, the temperature sinks from the surface 'to the bottom.'[1] Sir Wyville Thomson was doubtless more strongly confirmed in this opinion on having ascertained that the coldest is also the heaviest temperature in sea water of otherwise the same specific gravity. For this reason the extra expense and trouble of thermometers arranged for recording alternations at intermediate depths were considered unnecessary. This seemed to cut off one of the most interesting directions in which I had hoped that confirmation of my sections might be obtained. When the expedition had made considerable progress it was found that the thermometers, though not designed for the purpose, were in fact indicating alternations of

[1] *The Depths of the Sea*, by C. Wyville Thomson, 2nd edition, 1874, p. 300.

temperature, and special thermometers were then ordered; the latter were, however, either in consequence of imperfect mechanism or inconvenience of diverting from other work the necessary trouble and attention required by them, of no use; and records which had shown, or afterwards continued to show, alternations of temperature were 'corrected' in an arbitrary manner to temperatures intermediate between those of the thermometers above and below the 'discrepancy.' I do not think the records ought to have been 'corrected' in such a manner; and, even granting that course to have been justifiable, such treatment of the records ought to have been clearly explained, and the actual record published in the first instance. But instead of this being done, those 'corrected' records were published, in Admiralty Reports from 1875 onwards, as temperatures obtained from the thermometers, thus giving a very erroneous idea of the investigations. It was not until the year 1884 that the true record of temperatures obtained by the thermometer was published, showing the character of the 'corrections' which had been made on board the 'Challenger.' And it was not until some years afterwards that I chanced to look over the full records without expecting to find anything intrinsically different from what I had previously received from the Admiralty, but found a profusion of evidence of which I give the following specimens.

*First.*—The 'Challenger' steamed eastwards in the North Pacific from the coast of Japan for about 3,000 miles, and through seventeen consecutive soundings, extending east and west, the thermometer shows a break in the fall of temperature commencing at a depth of about 300 fathoms below the surface of the warm Japan current, and gradually falling in a waving line until the rise of temperature is shown quite at the bottom of the ocean. This alternation of temperature thus starts from near the surface where the warm waters of the Japan current interlace on the surface with the cold Arctic current, and falls in an easterly direction, at first rapidly and then in a waving line until, 3,000 miles to the east, the warmer streak of water is spread along the very bottom of the ocean. It is thus mere matter of fact that the 'Challenger' thermometers show an alternation of relatively warm water under a layer of colder water extending for a distance of 3,000 miles, exactly as it was insisted ought to exist according to my sections of the circulation. There is also a similar alternation at a lesser depth, starting from the same locality, at a depth of 175 fathoms and shown by every sounding for 700 miles, in which distance it sinks down to 800 fathoms. But this evidence was all blotted out on board the 'Challenger' by changing the figures all along those lines in such a manner as to make the records show higher temperatures at the lesser than at the greater depths.

The annexed Table I. (folded at the end of the book) shows all the temperatures for the first 1,500 miles from Japan, at which distance the warm streak, commencing at 300 fathoms depth in the Japan current on the west side, appears to reach the bottom of the ocean. Six out of the next nine soundings, extending over a further distance of 1,500 miles eastward beyond those given in the table, show a warmer temperature at the bottom of the ocean than some distance above, as in the last column of the table annexed. Higher up there is a confused alternation of temperatures due to the meeting of vast mid-ocean streams from the south and north, which are explained in the second part of this article; but the lower streak is connected through all the twenty-six soundings for the 3,000 miles. In three intermediate soundings it rises again after reaching the bottom, leaving the coldest water in that position, as indicated by the sections attached to the chart in 'The Ocean,' and this phenomenon will doubtless be found in each of the oceans.

It must, I think, be admitted that discrepancies for varying sluggishness or other action of the different thermometers, and for inaccurate adjustment or reading from the scales attached to them, are doubtlessly scattered throughout the records, as is made evident in the sounding 700 miles from Japan. It will, however, be seen that in the last four columns of the annexed Table I. there is no

TABLE II.—TEMPERATURES IN FAHRENHEIT DEGREES

NORTH LATITUDE — EQUATOR — SOUTH LATITUDE

| — | 11° N. | 9° N. | 7° N. | 5° N. | 2° N. | 1° S. | 3° S. | 5° S. | 7° S. | 11° S. | 13° S. | 16° S. | 17° S. | 17° S. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Surface . . | 80 | 80 | 81 | 81·2 | 79·5 | 78·7 | 79 | 80·7 | 80·2 | 80 | 80 | 79 | 79·5 | 79 |
| 10 fathoms . . | 74·9 | 76·7 | 79 | 80·4 | 78·8 | 77 | 78·8 | 80·7 | 80 | 79 | 78·9 | — | 77·8 | — |
| 20 ,, . . | **76** | 74 | **79·3** | 78 | 77·5 | 76·1 | 78·2 | 80 | 79·7 | **79·7** | 78·8 | — | 77·7 | — |
| 25 ,, . . | — | — | — | — | — | — | — | — | — | — | — | — | — | 75·8 |
| 30 ,, . . | **75·6** | 68·4 | **79·3** | 77·7 | 77·3 | 75·9 | 77·5 | 79·5 | **80·1** | **79·2** | 78·7 | — | 77·2 | — |
| 40 ,, . . | 70·3 | 63 | 78·2 | **77·8** | 75·7 | **76** | **78·2** | 79·4 | 79·7 | 79 | 78 | 75 | 76·8 | — |
| 50 ,, . . | 64·7 | 55·1 | 74 | **77·8** | 75·2 | **76·2** | **78·1** | 79·3 | 79·3 | **79·2** | **78·1** | **76·2** | **77·2** | **76·2** |
| 60 ,, . . | 58·6 | 53·6 | 63·4 | 77·5 | **75·3** | **76·2** | 77·5 | **79·8** | 79·1 | 78 | **78·2** | **75·8** | 76·8 | — |
| 70 ,, . . | 55·2 | 52·3 | 56·2 | 77 | **76·9** | 74·5 | 75·4 | 78·7 | **79·3** | 76·5 | 77·7 | 75 | 74·5 | — |
| 75 ,, . . | — | — | — | — | — | — | — | — | — | — | — | — | — | 74·4 |
| 80 ,, . . | 55 | 52·2 | 52·6 | 73·1 | 75 | 68·8 | 74 | 75·8 | 75·8 | 75·1 | 76·2 | 73·8 | 73·5 | — |
| 90 ,, . . | 53 | 51·6 | 51 | 66 | 67·2 | 63 | 67·2 | 71·9 | 74·5 | 72·7 | 74·1 | 72·7 | 71·8 | — |
| 100 ,, . . | 51·4 | **52·2** | 51 | 59 | **77·4** | 59·4 | 62·1 | 68·2 | 71·2 | 72·4 | 73 | 72·2 | 70·8 | 72·4 |

alternation of fall and rise shown in 1,200 fathoms of depth ; but it seems impossible to suppose that such errors as above alluded to do not exist in those records as well as in those of the next nine soundings, and the fact is that in each of those nine soundings alternations are shown and occur in most of them two or three times, making in all twenty alternations in the nine soundings, without counting the nine alternations which form the connected line at the lower depth. If such errors were all in one direction the state of the temperatures might be masked, but, as they cannot well be so, a more or less contorted shadow of the existing alternations is given.

*Secondly.*—In the temperate zone of the South Pacific, between the latitudes of 28° and 40° S., at seventeen consecutive stations the thermometers showed a constant fall of temperature through the first 100 fathoms in every instance. And they also showed a similar fall of temperature in the temperate zone of the North Pacific, between latitudes 20° and 38° N., through the same depth, in every instance at nine consecutive stations. But in the intervening equatorial regions, between latitudes 12° N. and 18° S., at fourteen consecutive stations an alternation of fall and rise was shown in every instance within that same depth, as shown by the annexed Table II. For instance, on Sept. 17 (16° S.) the observed temperatures were 75° F. at 40 fathoms and 76·2°

at 50 fathoms; and next day (17° S.) they were 76·8° at 40 fathoms and 77·2° at 50 fathoms. But in the Preliminary Report the figures are altered and declared to be, on September 17, 77° at 40 fathoms and 76·3° at 50 fathoms; and on September 18, 77·3° at 40 fathoms and 77·1° at 50 fathoms. Every one of the fourteen records is altered in the same manner as those two. I say that the manner in which each of the above fourteen soundings was corroborated by the other thirteen as regards showing the alternation of fall and rise of temperature is strong evidence that there really is such an alternation in the locality in which those fourteen soundings were made; and that it is not right that such phenomena should be ignored, as they have been throughout the 'Challenger' narrative, without better reason for so doing being given than has hitherto appeared. The cold water thrust upwards into the warm water along the equator tends to create such a confusion of temperatures as shown by the thermometers in that region.

In most of the above soundings in the temperate zones the temperatures are taken at intervals of 25 instead of 10 fathoms, at which they are taken in the equatorial regions; and this of course makes any existing rise of temperature more likely to be discovered in the latter regions; but, in fact, in those fourteen equatorial soundings there are fourteen records which show a temperature warmer than 20 fathoms higher up. One of these shows the warmer water 70 fathoms below the colder water, two others show it 40 fathoms below, and two more at 30 fathoms below. The horizontal differences also show the intrusion of the cold water, which causes the above confusion of temperatures. For instance, at about 6° N. there is at a depth of 80 fathoms 52° F. in juxtaposition with 73° F.; and at about 8° N. there is at a depth of 50 fathoms 55° F. in juxtaposition with 74° F. In latitude 7° N. the temperature falls 30° F. in the first 90 fathoms, whilst in latitude 7° S. the fall of temperature in the same depth is less than 6° F. These acknowledged differences all tend to confirm the vertical alternations shown by the thermometers, which I venture to think have been erroneously treated as errors. There doubtless are some errors among the observations, but, broadly considered, for all parts of the ocean it seems impossible to regard them otherwise than as caused by real alternations of temperature.

*Thirdly.*—In the North Pacific Ocean, in the Japan Current, latitude 34° N., longitude 140° E., the following temperatures were shown by the thermometers:—

At 125 fathoms, $54\frac{1}{4}$° F.
" 150 " $64\frac{1}{4}$° F.

And in a corresponding position in the Gulf

Stream in the North Atlantic, latitude 34° N., longitude 67° W. :—

| | | |
|---|---|---|
| At 125 fathoms, | | 45° F. |
| „ 150 | „ | $63\frac{3}{4}$° F. |
| „ 200 | „ | $64\frac{1}{4}$° F. |

Also at latitude 37° N., longitude 71° W. :—

| | | |
|---|---|---|
| At 550 fathoms, | | $35\frac{1}{4}$° F. |
| „ 800 | „ | 45° F. |

The above temperatures were changed on board the 'Challenger' in such a manner as to show the higher temperatures uppermost. I feel sure that but for the untimely deaths of the two principal leaders of the Admiralty expeditions they could not have continued to refrain from perceiving that the 'Challenger' thermometers had distinctly declared against their side of the controversy; and I do not see that the disaster of their loss should be a reason for the disaster of leaving the truth unacknowledged.

Out of 260 serial soundings, of which details are given in the Report published in 1884, 163 show an increase of temperature with an increase of depth, which had in most cases in the previous reports been recorded as a *decrease*, and in some instances left unreported. Some of those 163 serial soundings show alternations of fall and rise of temperature three or four times between the surface and bottom of the ocean. I do not object to the staff of the 'Challenger' expedition publishing to the world the temperatures which they think their thermometers ought to have recorded; but I say that they ought to have frankly stated that such records did *not* give the 'temperature obtained by 'the thermometer,' but that which, in their opinion, the thermometer ought to have given.

The foregoing evidence obviously suggests the question as to how it comes to pass that thermometers which were not supposed to be able to record alternations of temperature intermediate between the maximum and minimum temperatures passed through actually have furnished such records. This seems to be explained by experiments made by Commander Beardslie, U.S.N., with the 'Challenger' thermometers in an ice-bath of the temperature of 32½° F., in which a standard thermometer fell from a temperature of 70° to that of 32½° in one case in 20 *seconds*, and in another case in 40 *seconds*; and the 'Challen-'ger' thermometer took in the latter case 25 *minutes* to fall to only 35·1°, and in the former 14 *minutes* to fall to only 35°.[1] This seems to show that a 'Challenger' thermometer might *pass through* a cold stratum of water without recording it, whilst another higher up on the sounding line, chancing to *rest* in the cold stratum, would record it.

It may, of course, be taken for granted that

[1] *Deep-sea Soundings*, by Charles D. Sigsbie, U.S.N., Washington, 1880, p. 114.

those who treated the observed temperatures as above pointed out did so in good faith, really supposing that they were substituting for errors of the thermometers the true temperatures of the water. But it is, I think, to be regretted that my inquiries have so far failed to elicit an answer as to whether the scientific staff of the expedition still think that the observations as altered by them for the preliminary reports give, generally speaking, a more correct idea of the existing distribution of temperature than is given by the observed temperatures. It seems necessary to suppose that they do still think that there is the constant fall of temperature, as they have not done anything towards modifying the opinions expressed to that effect, and the sections in the diagrams throughout the 'Challenger' volumes show, excepting in the polar regions, a constant fall of temperature from the surface to the bottom all over the ocean, thus ignoring what will perhaps hereafter be considered one of the grandest results of the explorations.

In the annexed Table III. (folded at the end of the book) I have collected together, on the left-hand side, all the deep-sea records obtained by the 'Gazelle'[1] expedition in the South Atlantic between the equator and 7° S.; and, on the right-hand side, those obtained by the same expedition in the western basin of the South Atlantic south of the above-mentioned parallel of latitude. It will be seen that in the

[1] *Die Forschungreise S.M.S. 'Gazelle,'* Berlin, 1888, ii. 44.

former region not one of the serial soundings shows any of the so-called 'mistakes.' Each sounding in that region shows a constant fall of temperature from the surface. But in the more southerly part of the western basin every one of the eight soundings taken in that region shows an alternation of fall and rise of temperature between 500 and 1,200 fathoms. In one of those soundings three records at 500, 600, and 700 fathoms respectively each show a colder temperature than any of the four deeper records at 800, 900, 1,000, and 1,200 fathoms. It seems scarcely possible that the different results of the records in the two regions can be entirely due to accidental mistakes. At any rate, the difference between the two regions in Table III. tends to confirm the argument I have given above to the effect that the alternations of rise and fall of temperature shown by the 'Challenger' thermometers have erroneously been indiscriminately treated as mistakes.

The most popular theory of ocean currents was for a long time that which attributes them chiefly to differences of specific gravity; but as a result of the 'Challenger' explorations the chief of the expedition, Sir Wyville Thomson, in his 'Report to the Admiralty,' dated December 5, 1875, says: 'I have never seen, whether in the Atlantic, the 'Southern Sea, or the Pacific, the slightest ground 'for supposing that such a thing exists as a general 'vertical circulation of the water of the ocean 'depending upon difference of specific gravity.' In view of that authoritative statement I do not think it necessary in this paper to discuss the general merits of that theory with so much attention as those under which the action of the winds is now more plausibly advocated as the principal cause of ocean currents. As regards specific gravity, according to the records in the 'Challenger' 'Summary of Results,' the cold water shown in Table II. (p. 8) rising at 100 fathoms in latitudes 7° to 11° N. is heavier than the nearest warmer water on each side for which specific gravity is recorded, even when reduced to the same temperature; and therefore *in situ* the cold water must be decidedly the heavier. In some other parts of the ocean cold water rising from the lower strata is lighter than the warm water through which it rises. This confused distribution of specific gravities tends to confirm the argument of the seventeenth chapter of 'The Ocean,' to the effect that by the action of the earth's rotation all portions of the waters of the ocean are in their turn alternately exposed to the heat of equatorial and the cold of polar regions, so that the differences of specific gravity do not become sufficient to give them any important current-creating action. The current-creating action of the earth's rotation is quicker, and does not give that which would otherwise result from differences of specific gravity time to become effective.

The differences of absolute specific gravity in large masses of water in different parts of the great oceans is equivalent to a weight of about four grains in each 1,000 grains,[1] but, as it is the warm water which generally has the greatest absolute density, the actual difference in the ocean is generally reduced; and wherever a low absolute specific gravity of surface water is further reduced in the ocean by high temperature, it cannot have any direct tendency to cause a vertical circulation, but must rather tend to continue floating on the surface of the heavier water.

In Table I. (end of book) the discrepancies in the middle of the fourth column might at first sight be confidently treated as errors. But as the specific gravity records show the water in that column at 300 fathoms to be heavier than at 100 fathoms both above and below that depth, it seems not improbable that there may be alternations of temperature as well as of specific gravity in that locality, and that the discrepancies between thermometers at the same depth may be due to some of them being slower than others in changing with the alternations of temperature.

Specific gravities are not recorded for the depths of 125 and 150 fathoms in the first column of Table I., where an important alternation of temperature occurs; nor for the corresponding position in

[1] J. T. Buchanan, '*Challenger*' *Reports*, 'Physics and Chemistry,' i. part ii. 1.

the Gulf Stream of the Atlantic, where a similar alternation of temperature is recorded.

## PART II. FACTS AND CAUSES

One of the most important features of the oceanic circulation caused by the earth's axial rotation is the motion downwards and eastwards along the two temperate zones. This movement is supplied by water rising and moving westwards along the equatorial regions and in each of the polar regions, and then curving eastwards as it leaves each of those regions to join the downward motion of the temperate zones, from which it diverges in the under strata with an eastward drift towards the polar regions and a westward drift towards the equatorial regions. Excepting counter-currents, which will be presently considered, the foregoing is the course of circulation which the earth's rotation tends to cause.[1]

[1] The mathematical demonstrations of the theory of the above action are given in Propositions XXV., XXVII., X., and XI., of *The Ocean: its Tides and Currents and their Causes*, 2nd edition, Longmans, 1885.

My contention has been that vis inertiæ is incessantly endeavouring to restore an equilibrium as incessantly disturbed by the motive force which causes the earth to rotate, so that under the action of those forces the ocean can never be in a state of equilibrium. I did not until just now know that Todhunter points out that mathematicians have never demonstrated any manner in which gravitation and centrifugal force combine to produce equilibrium on the surface of a fluid ellipsoid of rotation, though its existence has been taken for granted, and the action of the forces producing it supposed to be demonstrated. I think the opinion given by Todhunter, p. 196, The motions eastwards in the temperate zones and westwards in the equatorial regions are in both cases very clearly marked, as the prevailing winds in each region tend to give those directions to the water, or, to say the least, do not tend in those localities to obliterate the true course of the circulation by creating a contrary surface drift; and the existence of those features of the circulation has long been recognised by all who have studied the subject.

The sinking of the water along the temperate zones, which is in theory quite as important as the horizontal motion, has until recently held a very different position as regards our knowledge of the subject, and the supposition that no such general downward motion in those zones existed was, in the year 1866, used as an argument to the effect that the theory which required such a motion could not be correct. The explorations by the 'Challenger,' the 'Tuscarora,' the 'Gazelle,' and others, all, however, accord in establishing the fact that the water sinks in the temperate zones and rises along the equatorial regions.

The temperatures shown by the 'Challenger' sections at 100 fathoms below the surface, in mid-ocean, are:

vol. i., of his *History of the Theories of Attraction*, published in 1873, and in other parts of the same work, ought to allow more consideration to be given to the simple demonstrations I have offered in my above-mentioned work. It is a mistake to suppose that there exists any mathematical demonstration at variance with them.

| | | |
|---|---|---|
| In the Atlantic, on the equator, | | 55° F. |
| | in latitude 20° N., | 65° |
| | „ 20° S., | 62° |
| In the Pacific, on the equator, | | 50° to 60° F. |
| | in latitude 20° N., | 60° to 70° |
| | „ 20° S., | 65° to 72° |
| In the Indian Ocean, on the equator, | | 56° to 60° F. |
| | in latitude 20° S., | 65° to 70° |
| | „ 20° N., | |

the diagram merely shows the temperature to be above 60° F., but at 200 fathoms it shows 52° on the equator and 56° in latitude 20° N.

The section for 200 fathoms shows in the Atlantic all along the equator a temperature of about 47° F., whilst in mid-ocean it shows in latitude 35° N. 60° F., and in latitude 35° S. 55° F.

All those undisputed facts are, on the one hand, completely at variance with what was in the year 1866 supposed to be the distribution of temperature; and, on the other hand, they clearly accord with the sections which I submitted to the Council of the Royal Geographical Society in that year.

The eastward motion of the water as it sinks is shown by comparing sections at different depths. In the North Atlantic at the depth of 300 fathoms the warmest water, 63° F., is found about latitude 30° N., longitude 62° W., which is in the temperate zone on the *west* side of the ocean; whereas at the depth of 600 fathoms the warmest water, 54° F., is found in latitude 38° N., longitude 12° W., also in the temperate zone, but on the *east* side of the ocean.

The above temperatures are all taken from Dr. Buchan's diagrams published with the 'Challenger' Reports.[1]

The surface motion from the polar regions to the temperate zones, supplied by water rising in the former and sinking in the latter, is shown throughout the Great Southern Ocean by the movements of icebergs and ice-floes, which drift north-westwards in the far south and then turn northwards and eastwards as they approach the temperate zone, where their northward course is stopped by the sinking of the water in which they float, and by the pressure of the water from the equatorial regions, which sinks together with the water which brings the ice from the south.

The northward movement of the ice shows the great preponderance of the force by which it is impelled over that of the winds; for the prevalent winds diverge along the surface of the ocean from the temperate zones towards the equator and the poles, so that the ice travels northwards through the latitudes in which the general course of the winds is from the north, and it then ceases its northward course as it enters the zone from which

[1] *Scientific Results of the Voyage of H.M.S. 'Challenger,'* second part, 1895, Appendix.

the prevalent winds commence to blow from south to north. But for the sinking of the polar water in the temperate zones, Arctic and Antarctic icebergs would doubtless often meet in the equatorial regions.

The configuration of land and water makes the phenomena of the circulation more complicated in the Arctic than in the Antarctic regions. In the latter regions the water has comparatively free access to converge from all directions and to flow north-westwards and curve round to a north-eastward course. But the continents around the Arctic Ocean have access from the temperate zone in two directions only; and, of those two openings, the one by Behring Strait is so narrow and shallow as to be unimportant as compared with the opening from the Atlantic, on the east side of which, off the coast of Norway, a vast mass of water is poured into the Arctic basin, deranging the normal circulation there, and then returning south on the west side of the ocean off the coasts of Greenland and Labrador. The drift south-westwards along the surface may probably be found to prevail in the unexplored regions of the far north. It seems to have stopped the northward drift of the 'Fram' in about latitude 85° N. and turned it south-westwards. It impeded Parry's sledge journey in latitude 82·45° N. by carrying the ice south-westwards almost as fast as the sledges could be dragged northwards over it. It creates a surface drift south-westwards from the western shores of Iceland, from the western shores of Greenland in Davis Strait, Baffin's Bay, and Smith's Sound; and from the western coast of Alaska south of Behring Strait. Those evidences tend to show that if the Arctic Ocean were completely land-locked the prevalent surface drift would be south-westwards; and along the shores of the continent the course of the ice-drift would therefore be from east to west; whereas that circulation is to a great extent reversed by the mass of water which is poured into the Arctic basin, chiefly by an undercurrent, from the western coast of Norway. Before the eastern extension of the Paleocrystic Sea was discovered by Nares, or the still greater extent of the Arctic Ocean by Nansen, the fact that warm undercurrents from the Norwegian coast traversed the Arctic Ocean was made evident by the warm water found about Herald Island and Wrangel Land, to the north of Behring Strait, and also by the eastward pressure of the ice in the channels of the North American coast. The preponderant mass of water poured in as an undercurrent from the Atlantic tends to rise against the northern shores of Asia and flow northward from thence, reversing the normal south-west drift. The accumulation of much older ice-floes off the American than off the Asiatic coast was at one time supposed to show that Greenland must extend to the pole and thus stop the free escape of the ice eastwards; but the normal surface pressure from the north-east, meeting what may be called the abnormal pressure of the water forced in from the east side of the North Atlantic, and drawn back on the west side, seems sufficiently to account for the accumulation of masses of ice in the Paleocrystic Sea extending westwards from Smith's Sound towards Behring Strait.

The surface water diverging from the equator with a direction first westwards and then curving eastwards shows the immense preponderance of the actual forces of oceanic circulation over the current-creating action of the winds; for in each ocean it flows first across and then right against the strongest parts of the trade winds. In the North Atlantic there are great quantities of the Sargasso seaweed floating on the surface of the water in the northern part of the trade-wind region; but the surface current flowing north-westwards from the equator and then turning northwards and eastwards, passing on both sides of the Azores, drives back the floating weed, which would otherwise be drifted along with the wind down to the northern coast of Brazil and into the Caribbean Sea. Further evidence to the same effect is given by the fact that if, as has been asserted, the trade winds were the cause of the Gulf Stream, by driving the surface water into the Caribbean Sea and Gulf of Mexico, there would be some tendency of the water, thus heaped up in those seas by the winds, to escape back as undercurrents as well as in the Gulf Stream; but, instead of this being the case, it has, on the contrary, been shown by the United States Coast Survey that there is less surface flow in the Caribbean Sea and Gulf of Mexico than from them; and also Captain Maury's temperature charts clearly show the rising of water from cold undercurrents mixing with the warm surface water in the Caribbean Sea and also in the south-west and west central parts of the Gulf of Mexico. The cold undercurrent which enters the Caribbean Sea and Gulf of Mexico is part of the normal undercurrent of the ocean which corresponds with the normal undercurrent of the atmosphere which forms the trade winds.

In the South Atlantic, the island of St. Helena lies in the strongest line of the trade wind of that ocean, and it has long been known that sailing vessels easily beat to windward there; [1] which seems to me to show that they are assisted by the oceanic flow, which bears them up against the wind in the same manner as in the central parts of the North Atlantic the similar surface flow assists vessels to beat northwards against the north-east trade wind.[2] The warm water which flows past St. Helena sinks downwards when it reaches latitudes far south of that island. It was crossed by the 'Gazelle'

[1] *United States Exploring Expedition*, by Charles Wilkes, U.S.N. London, 1845, v. 459.

[2] *An Investigation of the Currents of the Atlantic Ocean*, by Major James Rennell, F.R.S., 1832, p. 115.

about latitude 34° S., longitude 3° W.; and by the 'Challenger' about latitude 36° S., longitude 6° to 10° E.; it is soon after this submerged by the cold Antarctic drift from the south-west; warm water eddies again to the surface a short distance south of the last-mentioned position, but is probably from part of the main stream submerged farther west; and the greater parts of both no doubt continue their course as an undercurrent partly eastwards and partly with an eastward drift to the far south. The pressure of this stream bears back the icebergs in the South Atlantic and bars the course by which they would otherwise wend their way to the Gulf of Guinea. Conditions similar to those met with at St. Helena are found in the trade-wind regions of each ocean. The trade winds converge towards the equator, but the surface water diverges, running across and against the winds in both directions. At the Sandwich Islands in the North Pacific, as at St. Helena in the South Atlantic, there is rarely any difficulty in beating to windward.[1] Though both islands are in the trade winds, the ocean drift does not flow with the wind in either case. At the Society Islands, in the South Pacific, the trade wind is not so constant as at St. Helena and the Sandwich Islands. During a prevalence of calm weather the water has been known to form a rapid current setting to the southward.[2] This seems to show

[1] Wilkes, *U.S. Exploring Expedition*, v. 476. [2] *Ibid.* iv. 276.

the normal current, unaffected by the wind. Where the trade wind blows against it the surface motion is not so apparent, but, as above shown, it does not appear generally to be reversed even where the trade winds blow most strongly and steadily. This same surface flow from the equator carries warm water southwards against the steadiest part of the trade wind in the more easterly regions of the South Pacific. Some of this warm water was traversed by the 'Challenger' about longitude 90° W., latitude 38° S. And in the steady part of the trade-wind regions to the north-east of that position through sixteen degrees of latitude Captain Lutké [1] found no current; and Captain Wilkes [2] experienced very little, and what little current existed was against rather than with the trade wind. In some localities the trade winds certainly do drive the surface water along with them; but it seems to me, the fact that the surface water is not generally swept rapidly to leeward in those trade-wind regions is evidence of the existence of a cause for a surface flow in the opposite direction. Mr. Findlay, in commenting

[1] 'En latitude de 26° nous reçûmes un vent de S.E., qui passa insensiblement à l'état de véritable vent alizé, et qui même quelquefois souffla fraîchement; mais tout cela ne produisit point de courant; pendant deux ou trois jours différens nous eûmes un faible courant d'O. suivant le vent, et pendant autant de jours retournant contre le vent. Dans le cours de ces deux semaines la différence entre la longitude estimée et chronométrique ne dépassa pas 20′, et il n'y en avait non plus aucune en latitude.'—*Voyage autour du Monde*, Frédéric Lutké, 'Partie Nautique' (Saint-Pétersbourg, 1836), p. 186.

[2] *U.S. Exploring Expedition*, v. 471.

on these equatorial currents of the South Pacific, says: 'Why it is that the currents should be in 'many cases quite imperceptible, or even flowing in 'a contrary direction to that which all reasoning 'and analogy drawn from known facts would 'indicate, is a problem yet to be solved.' [1]

The general tendency of cold undercurrents to rise in the neighbourhood of the equator in all the oceans makes the water in many places colder than a few degrees from the equator both north and south, as the water further from the equator has been longer on the surface and had more time to get warmed by the tropical sun. On the meridians of 150° to 160° W., about the middle of the Pacific Ocean, the water is usually colder for about three degrees on each side of the equator than in latitudes 5° to 10° N. and 5° to 10° S. The temperatures recorded by the 'Challenger' are:—

| | |
|---|---|
| On the equator . . . . . | 78° to 79·5° F. |
| In latitude 5° to 10° N. | 80° to 81·2° |
| „ 5° to 10° S. | 80° to 80·7° |

The same phenomena occur in the central parts of the Atlantic. A temperature of 80° and upwards is there sometimes found at a distance of 10° from the equator on both sides, when between latitude 10° N. and 10° S. the temperature is all less, and sometimes much less. In the Indian Ocean, part of the undercurrent from the south

[1] *South Pacific Directory*, p. 1124.

seems to pass the equator and rise along the north-western shores of the Arabian Sea, in consequence of not meeting at the equator a similar stream from the north, in much the same manner as part of the North Atlantic undercurrent which enters the Arctic Ocean flows on towards Behring Strait in consequence of not being met by an equal pressure from that direction. But most of the undercurrent evidently rises along the equator, making the phenomena of the Indian Ocean in general similar to those of the Atlantic and Pacific.

There are, in each of the oceans, indications of an important counter-current, which the pressure of the normal surface flow from the equator to the temperate zone tends to create in the following manner. The form of the curve which the surface water tends to describe in its course from the equator to the temperate zones is dependent on the velocity of the earth's rotation, and the water which diverges from the equator in the central part of the ocean has comparative freedom to follow the normal curve so determined; whereas on the west side of the ocean the land stops the westward motion and compresses the water into a stronger current, which presses against the central flow as the latter follows its normal course. There is in each of the six oceans a counter-current starting from the meeting-point of two currents which both have their origin in the equatorial regions.

In the North Atlantic this counter-current is clearly marked and increased by the action of the Arctic stream which bears down from the north against the two equatorial streams and forms a vertical eddy, which rises to the surface between the two equatorial streams, intermingling with a rapid eddy of warm water which runs westwards from the meeting-point. This warm eddy, joining the cold water upheaved between the two main currents, is always to be found in that locality. The mid-ocean stream is itself often subdivided by a somewhat similar eddy south of the Azores, making a clear distinction between a compact mass of water which streams north-eastwards to the east of the eddy and the main stream, which passes northwards on the west of the Azores and continues its course north-eastwards. The pressure caused by the narrowing of the latitudes must tend to facilitate the formation of such vertical eddies when once started by any initial cause.

In the South Atlantic, 14° W. and 38° 40′ S., Captain Grant writes: 'Why the water should be '*warmer* here than between the parallels of 35° and '37° S. is a problem that, in my mind, admits not 'of so easy solution, especially if my suspicions are 'true in regard to the northerly set.' [1] About three degrees farther north, Captain Vonck 'found his 'ship drifting quickly nothwards;' and says: 'I

[1] Maury, *Physical Geography of the Sea*, sect. 750.

'have met this current here on all my voyages.'[1] Records of currents and of temperature show this to be part of a normal counter-current which divides the Brazil coast current from the warm water which streams southwards through mid-ocean. There are indications that this mid-ocean stream is itself divided by a northward drift in latitude 20° S., separating the main central stream from that which passes St. Helena. The relative configuration of the oceans prevents these counter-currents from being so important in the South as in the North Atlantic and in the North and South Pacific Oceans; but they nevertheless exist in the South Atlantic and South Indian Oceans. The pressure in the temperate zones, caused by the meeting of the surface flow from the poles and from the equator is enhanced by the narrowing of the latitudes into which the warm flow from the equator passes, and thus gives the undercurrents a constant tendency to rise in vertical eddies, dividing the warm flow as it streams from the equator wherever irregularities of depth most facilitate the upheaval of the undercurrent.

There are important currents near the equator which have in each ocean the title of the equatorial counter-current, because they run eastwards counter to the general drift of the ocean on each side of them. Their usual positions are north of the equator; and, as the average meeting of the

[1] Board of Trade, *Charts of the South Atlantic*, 1869, p. v.

north-east and south-east trade winds is also north of the equator, a plausible explanation which suggests itself to account for them is that the trade winds sweep the surface water along with them up to each edge of the calm belt which lies between them ; and the counter-current is then created by part of this water escaping back eastwards in the calm belt. If the eastward currents were in fact found to be characteristic features of the calm belt, the two phenomena might well be regarded as cause and effect. But the fact is that in the open ocean, where the current is free to sway northwards and southwards with the calm belt, it does not seem to show any tendency to do so. The north-east trade wind blows over both the eastward and the westward current; and the counter-current runs quite as much against the trade wind as in the calm belt. In the western part of the Atlantic this current is very irregular, and is found close to the equator near the middle of the ocean and farther north to the west of that position, just as if it formed the tail end of the counter-current already described as dividing the mid-ocean stream from the Gulf Stream farther north. Captain Mann, F.R.G.S. and F.R.A.S., records that between 52° W., 8° N., and 27° W., on the equator, with the south-east trade wind blowing, he found a rapid current throughout all that distance running against the wind.[1] As the mid-ocean stream

[1] Findlay, *North Atlantic Memoir*, 12th edition, p. 285.

runs across and against the north-east trade, and this counter-current itself runs against the wind as often as between the winds, the wind and the current are not practically so easily reconcilable as cause and effect as they are theoretically.

In the Pacific, this counter-current is much more constant and extensive than in the Atlantic Ocean. Throughout the central and western parts of the ocean it seems to be a normal current generally between the fourth and sixth degrees of N. latitude, but also at times as far south as the equator. In the month of July, in longitude 175° E., on the equator, Admiral Lutké found a current of two to three knots an hour running to the eastward for fourteen or fifteen days, although the wind was then fresh from the eastward.[1] In the month of August the U.S. ship 'Narragansett' had the north-east trade wind on the passage from the Gilbert Islands, on the equator, to the Marshall Islands, in latitude 10 N., and passed, without change of wind, from the south equatorial current through the counter-current into the north equatorial current.[2]

Captain Maury declared that he could not understand the calm-belt currents of the Pacific Ocean; that they run at times with great force, and generally to the west.[3] That statement, by

[1] Findlay, *North Pacific Directory*, 3rd edition, p. 1200.

[2] *Ibid.*

[3] *Physical Geography of the Sea*, 8th edition, p. 195.

so experienced an observer, is strong evidence against the idea of the eastward counter-current being the normal current of the equatorial calm belt of the Pacific.

The diverging tendency of the equatorial current makes it, as it strikes the western coast of the ocean, practically two separate currents, which tend to eddy back against each other on the coast-line, and then to form a surface counter-current over the water which rises from the under strata to form the great surface flow of the equatorial current on both sides of the equator. The tendency of this water to diverge to the north and south as it rises along the equator may, in fact, facilitate the extension eastwards of a narrow surface current, originating in the eddies as just explained; but the existing counter-currents of the Pacific seem to be a very extensive development for so slight a cause; and there are two other physical actions which may perhaps contribute towards determining the position and increasing the extent of these counter-currents.

I have elsewhere given an accumulation of evidence showing that the surface of the earth, together with the level of the ocean, is raised in the temperate zone of the northern hemisphere above the normal level of the ellipsoid which would result from the sole action of the earth's axial rotation; and suggested that this seems to be due to exactly such a pressure from south to north as would naturally result from a motion of the earth southwards through space. Such a pressure might perhaps partly account for the fact of the counter-current, formed as above described, being found generally north of the equator, instead of along that line, which theory would otherwise indicate to be its normal position; but the chief cause is probably the difference in the distribution of land and water in the northern and southern hemispheres, which makes the undercurrent from the south preponderate and carry the mean line of their confluence north of the equator. This action would also make it easier for the counter-currents of the temperate zones to reach and unite with the equatorial counter-current from the north than from the south temperate zone. The section in Table I. shows, in the fourth column, the most westerly of the counter-currents of the north temperate zone in the Pacific, which is probably the principal source which supplies the equatorial counter-current in that ocean. In consequence of the difference in the configuration of the ocean, the corresponding counter-current from the south temperate zone is borne back by the surface flow from the equator and prevented from uniting with the equatorial counter-current.

Also, I have pointed out that though the ocean, resting on the solid surface of the earth, is dragged round with the earth's axial rotation by the force of gravitation, which holds it to the earth's surface, the movements of the fluid surface of Jupiter show that it is not dragged round in the same manner, but that the revolving force which causes that planet to rotate acts directly on the fluid surface, and carries it eastwards in the equatorial regions, making it revolve in a shorter period than the solid body of that planet. It seems quite possible that a similar action of the earth's revolving force may facilitate or assist the eastward motion of the earth's equatorial counter-currents after these latter have been given an impetus, and, in fact, a natural trough to run in, by the action of other causes. The evidence I have above alluded to regarding the shape of the earth seems to me to show that the solid surface is so pliant that a faster velocity of rotation would lift that surface just as much as the ocean in the equatorial regions. But supposing the earth to be sufficiently rigid to resist that change of shape, then an increasing velocity of rotation would increase the velocity and dimensions of the equatorial counter-currents, and of all the cold counter-currents which rise from the under strata dividing the warm surface flow which pours away from the equator to the temperate zones. Those counter-currents are, under existing conditions, generally borne back by the predominance of the surface flow westwards and from the equator; but a faster rotation would make them predominate and press eastwards towards the equator to unite with the equatorial counter-current and form channels of circulation which, with an increasing velocity of rotation, would gradually drain away the water from extra-tropical latitudes, and lift the whole ocean into that counter-current, so as to make it form a belt revolving high above the equatorial regions, with a shorter period of rotation than that of the earth itself.[1]

[1] Since the above essay was written I have presented to the Map Room of the Geographical Society the following charts, giving a great deal of detail of the circulation, together with explanatory letters, under the title of *Ocean Cyclones*:—

1. The Arabian Sea. August 10, 1898.
2. The Atlantic Coast from the British Isles to the Mediterranean Sea. August 17, 1898.
3. The North Atlantic Ocean. August 29, 1898.
4. The North Pacific Ocean. September 12, 1898.
5. The South Atlantic Ocean. September 26, 1898.
6. The North Atlantic: Supplementary Notes. October 7, 1898.
7. The Tides on a Chart of the World. November 8, 1898.

## *SECOND ESSAY*

### THE SEPARATION OF VIS INERTIÆ AND MOMENTUM

When Galileo and other philosophers of his time discarded the then prevailing idea of the earth being a fixed and immovable centre round which the universe revolves, and adopted, instead of that idea, the system which had a short time previously been expounded by Copernicus more elaborately than by any of the philosophers of more ancient times, declaring the earth to be incessantly in rapid motion circling round the sun, one of the objections urged against these disturbers of opinions then accepted as doctrine by all the leading seats of learning was to the effect that it was obviously impossible for the earth to have been moving in such a manner throughout all the period of recorded history, because, as there was nothing in space to keep it moving, it would, even if it had ever been in motion, long before that time have stopped, or at least have shown some reduction of velocity in the necessary process of spending its momentum.

To that argument the Copernicans might quite reasonably have replied by urging their arguments in proof of the fact of the earth being in motion, whilst repudiating the idea that because they discovered and showed the existence of that motion they were therefore under any obligation immediately also to discover the force by which the motion is caused. Instead, however, of limiting themselves to that argument, they replied to the effect that if there is nothing in space to keep the earth moving, it is just as true that there is nothing there to stop it, and, having once been set in motion by some cause no longer apparent, it therefore must, in the absence of any obstruction, continue for ever in motion. In this manner the new philosophy, of which Galileo and Descartes were leading exponents, gradually adopted as a 'law of nature' the idea that matter is indifferent to rest or motion. That is to say, that, if at rest, it will not move unless acted on by an extraneous force, and, if in motion, it will not cease to move unless impeded by an extraneous force. From a purely theoretical point of view, the argument is unassailable; but the question which remains open for consideration is as to whether the practical evidence of physical phenomena supports that theory more than that which, from a purely theoretical point of view, is equally plausible, and has been advocated by Leibnitz and many others, to the effect that matter has an inherent tendency to be in motion, and that motion is therefore its natural state; or whether a third theory, equally reasonable as pure theory, may not be most completely in accord with the evidence of physical phenomena—namely, that which had previously been generally accepted as a law of nature, and was adhered to by Kepler notwithstanding his acceptance of the Copernican System, to the effect that matter has an inherent tendency to be at rest, so that a body will not move unless set in motion by some extraneous force, and when the extraneous motive force ceases to act the momentum imparted by it will sooner or later be exhausted, and the body will revert to its natural state of rest.

The question as to the relative merits of those three theories is, I say, one to be decided by the evidence of natural phenomena, and not by such abstract arguments as those by which philosophy has attempted to show the idea of indifference to rest or motion to be a theoretical necessity, thus, in effect, declaring the other two rival theories to be shown by pure reason to be mathematical impossibilities. It is, I say, matter of fact, and not abstract argument, that must be appealed to for a decision as to whether matter has or has not been created with a natural tendency to be in motion; whether it has or has not been created with a natural tendency to be at rest; or whether it has or has not been created with a natural indifference to rest or motion. The last of those three conflicting opinions had already been adopted in the New Philosophy as a 'law of nature,' when Newton produced his mathematical demonstration of the action of the sun's gravitation as a centripetal force, controlling the motion of the earth round the sun. The existence of this centripetal force, constantly tending to draw the earth towards the sun, makes it obvious that there must be an equal centrifugal force which keeps the earth at its mean distance from the sun; and the asserted tendency of vis inertiæ to perpetuate momentum supplied the centrifugal force required to maintain equilibrium in the orbital motion. Thus Newton adopted the doctrine of indifference to rest or motion, for the purpose of explaining not only the continuous motion of the earth, but also its resistance to the centripetal force of the sun's gravitation. This latter was the purpose which made that new doctrine most immediately requisite to him. But both purposes are perfectly served by his method of treating the subject. In Definition III., book i., of 'The Principia' he says:[1] 'The vis insita, or innate force of matter, 'is a power of resisting, by which every body, as 'much as in it lies, endeavours to persevere in its 'present state, whether it be of rest, or of moving

[1] *The Mathematical Principles of Natural Philosophy*, by Sir Isaac Newton. Translated into English by Andrew Motte. London, 1819.

'uniformly forward in a right line. . . . This vis 'insita may, by a most significant name, be called 'vis inertiæ.' And in Definition IV. he says: 'A 'body maintains every new state it acquires by its 'vis inertiæ only.'

In conformity with the foregoing definitions, he frames his first axiom, or Law of Motion, in the same book as: 'Every body perseveres in its state 'of rest or of uniform motion in a right line, unless 'it is compelled to change that state by forces im'pressed thereon.'

According to that first Law of Motion and the definition of vis inertiæ which precedes it, the latter force tends to perpetuate any given motion; so that a force of momentum sufficient to resist the sun's centripetal force at the instant of the initial force being brought to bear to set the earth in motion, continues, according to that law, to repeat its work incessantly without any further action of the initial motive force being requisite to maintain the original power of momentum. In opposition to that idea I contend that Newton's laws of gravitation themselves supply not only the *centripetal* force, as demonstrated by him, but also the *centrifugal* and the *motive* forces, so that the idea of the vis inertiæ of matter giving it an inherent tendency to perpetuate momentum imparted by an extraneous force is not wanted for the only purpose for which it was invented. The momentum of a body in motion is in fact resisted by its vis inertiæ, so that Newton's definition embraces under the same term two not only distinctly different, but even antagonistic forces.

Suppose, for sake of argument, that the motions of the planets are determined entirely by the action of gravitation, and that the sun's gravitation is the force which revolves them. Then the motion of any planet along its orbit represents the action of the sun's revolving force of gravitation, and the lagging of the planets backwards over the surface of the sun represents the action of surrounding forces of gravitation which resist the sun's revolving action, and prevent the planets from keeping pace with the sun's rotation. The relative action of those opposing forces along each orbit ought, in such case, to accord with the relative force of the sun's revolving action in each orbit; and the following considerations show that such accordance does exist.

Any object resting on the surface of the Earth is held to that surface by the Earth's gravitation, and, therefore, revolves with the surface upon which it rests. But the planets are in fact held high above the moving surface of the sun, and if the reciprocal action of gravitation gives them a tendency to follow the motion of the moving surface below them, that tendency must be partially counteracted by a similar tendency to follow the motion of the opposite surface of the sun moving in the opposite direction; and the amount of this revolving force and the resulting motion must depend on the amount by which the power of the gravitation of the nearer exceeds that of the remoter hemisphere of the sun.

The existing motions are such that the amounts by which they differ do in fact accord with the relative amounts of that revolving force of gravitation in each orbit. For at any given point the force of gravitation exerted by any particle in the nearer hemisphere of the sun exceeds that of a particle in a corresponding position in the opposite hemisphere inversely as the cube of the distance from the centre of the sun, and the proportion which the actual velocities of the planets along their orbits bear to the lagging is approximately also inversely as the cube of that distance.

The above two points are shown by the following figures.

The ratio of velocities of the Earth and Neptune is as follows:—

| — | Periods of orbital revolution in days | Mean daily motion | Ratios of velocities to periods of revolution | Same, one equal ratio in orbit of Neptune |
|---|---|---|---|---|
| Earth . | 365·25 | 3,548″·19 | 9·714 | 27,100 |
| Neptune . | 60,126 | 21″·554 | ·0003584 | 1 |

Granted two different velocities of revolution, the ratio of periods to velocities must be as the square of that difference, but might mathematically, in the absence of any physical cause to the contrary, exist with the two bodies at the same, or at any different relative distances; but in fact that ratio of velocities making the Earth's ratio exceed that of Neptune as 27,100 to 1 is approximately as the inverse cube of the relative distances from the sun, for the Earth's distance is to that of Neptune as 1 is to 30·0368, by Sir John Herschel's estimate.

The most recent observations of the sun's parallax show the mean distance of the Earth from the sun to be about 92,700,000 miles, that of Neptune about 2,784,000,000 miles, and the diameter of the sun 865,000 miles. These figures make the relative distances from the nearest and furthest points of the sun respectively as follows:—

| | | |
|---|---|---|
| Earth . . . . | 92,267,500 | 93,132,500 |
| Neptune . . . | 2,783,567,500 | 2,784,432,500 |

And the relative distances inversely are therefore as follows: the sun's diameter being taken as unity, representing the difference in the orbit of Neptune, and fractions being discarded:—

| | | |
|---|---|---|
| Earth . . . . . . . | 97,110 | 96,208 |
| Neptune . . . . . | 3,219 | 3,218 |

The squares of the above inverse distances show the relative amounts of force of gravitation at each distance, and are as follows:—

| | | |
|---|---|---|
| Earth . . . | 9,430,352,100 | 9,255,979,264 |
| Neptune . . | 10,361,961 | 10,355,524 |

The differences of the above forces in the orbits of the Earth and Neptune respectively are 174,372,836 and 6,437; or as 27,089 to 1.

The above figures show that the amount by which the gravitation of the nearest exceeds that of the remotest part of the sun represents approximately the ratio which the velocity of the onward motion of any planet along its orbit bears to its apparent lagging backwards over the surface of the sun. And, as the same argument applies to any corresponding parts of the nearer and remoter hemisphere, they show that the ratio of velocities and the ratio of forces are both in each orbit inversely as the cube of the distance from the sun approximately.

For the distances are as 1 is to 30·0368.
The inverse cubes are as 27,099 is to 1.
The ratios of velocities are as 27,100 is to 1.
And the ratios of forces are as 27,089 is to 1.

Any of the other planets would serve just as well as the two above given to illustrate the subject, as is fully shown in 'The Ocean,' 2nd ed., chap. vii. and chap. xxi. Prop. xix., where I have based arguments on the real velocities of motion as well as on the apparent velocities dealt with above.

The estimates of 92,700,000 miles for the Earth's distance from the sun, and 865,000 miles for the sun's diameter, I have taken from Sir Robert Ball's

'Story of the Sun,' published in 1893; and Neptune's distance, 2,784,000,000 miles (not given in that work), is based on Sir John Herschel's estimate of the relative distances of the two planets.

The accordance of the velocities of motion with the theoretical action of gravitation is just as strong an argument in favour of the theory that the onward motion of the Earth in its orbit and the sun's gravitation are connected as cause and effect, as is the accordance of the Earth's centripetal tendency in its orbit with the sun's direct force of gravitation in favour of, and which has been accepted as proof of, the theory that the Earth's motion in its orbit is controlled by the sun's gravitation. If the fact that the distance, B C, in the figure annexed, which the Earth deviates in any given time from the tangent A B, accords with the calculated action of the sun's direct force of

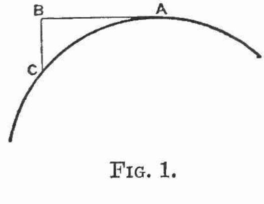


FIG. 1.

gravitation, is a proof of the action of that direct force, then the fact that the distance, A C, which the Earth travels in that same time along its orbit agrees with the calculated action of the sun's revolving force of gravitation is also a proof of the action of that revolving force. This revolving force of solar gravitation gives a definite reason for the onward motion of the Earth, which makes it no longer necessary for us to imagine the existence of an innate tendency to continue a motion imparted at some remote but unknown date by some unknown cause.

As the sun's gravitation moves the Earth, the connecting links of gravitation between the Earth and other bodies resist that motion because, the earth being at any moment in the position in which the joint action of all forces of gravitation tends to hold it; therefore, as the sun's gravitation moves the Earth from any point, it is opposed by the combined action of all other forces of gravitation, which tends to draw the Earth back in the opposite direction to that in which it is carried by the sun's gravitation. This resisting, or retarding force, as it draws in the opposite direction to that of the motion of the Earth, forms at the same time a centrifugal force drawing back at a tangent to the orbit in which the Earth is revolved, so that, as the sun's force of gravitation forms the centripetal force, the retarding action of the remaining force of gravitation forms the centrifugal force. And between the two the Earth is carried along the line of its orbit.

The difference between the action of a centrifugal force acting onwards at a tangent according to the prevailing idea, and that of one acting backwards at a tangent according to the new idea, seems at first sight so great as to make it appear that the question might easily be brought to a practical test so as to decide which theory is true. But the retarding force drawing backwards at a tangent is each moment drawing towards the tangent along which it has been supposed that vis inertiæ tends to carry the Earth. That onward tangent is the line along which the momentum of the Earth's orbital motion would carry it if the controlling force of gravitation suddenly ceased to act. And therefore the direction in which the Earth would move in case of the centrifugal exceeding the centripetal force is not in question. The question is as to the nature of the centrifugal force. To understand this point: first, suppose a wet mop is twirled. Then the drops of water on it are not only given an onward motion in the direction of the twirl, but if the velocity be sufficiently great, a centrifugal force is generated which carries them off at a tangent from the mop in the direction of its rotation. Now, suppose the sun to be motionless, and the Earth at rest on its surface. Then apply the revolving force of solar gravitation with sufficient velocity to throw the Earth off as a drop of water is thrown from a twirling mop. The forces acting in those two cases are intrinsically identical, though as regards the mop, the drops of water thrown from it come immediately under the influence of the preponderant force of the Earth's gravitation, and are by that force drawn away from the vicinity of the mop, so that the stoppage of the rotation of the latter cannot bring the drops of water back to their position on it; but the planets are all within the sphere in which the sun is the preponderant force of gravitation; and, as regards the Earth resting on the surface of the sun, as the latter is made to rotate, its gravitation tends to carry the Earth round on its surface with the same motion, but the combined action of all other forces of gravitation, resisting the effort made to move the Earth from its position, draws back at a tangent to the circle in which the sun's gravitation endeavours to carry the Earth, and whilst thus retarding the Earth's motion, carries it off at a tangent from the surface of the sun in the direction of rotation, just as the drops of water are thrown off from the mop. The Earth as it leaves the sun rises along a tangent to the sun's surface in the direction of its motion; but the point of the sun's surface from which it was thrown off has in the meantime passed on, leaving the earth also in a tangent to that point in the *opposite direction* to the motion of the sun's surface, because the surrounding force of gravitation has resisted the motion and prevented the Earth from keeping pace with it. The momentum is the cause of the centrifugal force; but that force of momentum is sustained by the constant action of the sun's revolving force of gravitation and is guided and controlled between the sun's centripetal force and the surrounding force of astral gravitation, which acts as a centrifugal force as it draws backwards in resisting the motive action of the sun.

We have seen that the revolving force of the sun's gravitation changes inversely as the cube of the distance, whereas its direct force changes only inversely as the square of the distance. And therefore, however great the excess of the centrifugal force may be at any point, the earth's recession from the sun under its action must at length bring the centrifugal into equilibrium with the centripetal force. If the sun's velocity of rotation were sufficiently increased, then the Earth would be thrown so far from the sun as to come under the predominant influence of some other power of gravitation, just as the drops of water thrown from the mop come under the influence of the Earth's gravitation, and it would then fall to that power just as the drops of water thrown from the mop fall to the Earth. But in fact the Earth and planets are within the sphere in which the sun's gravitation is the preponderant power, so that a stoppage of the sun's rotation would allow them all to fall towards the sun.

As regards the drops of water thrown from the mop, it is said that vis inertiæ tends to cause them also to move for ever onwards along the tangents on which they leave the mop, and that they are only prevented from effecting this by the obstruction of the air, or whatever they may strike. But then the particles of air struck by the drops of water cannot be less disposed to move for ever than the drops of water themselves. Is it not evident that the vis inertiæ of the air, which endeavours to keep it at rest, resists the motion of the drops of water? and, if so, then also the vis inertiæ of the drops of water must resist the motion; for there can be no more reason for supposing that the vis inertiæ which stops the motion resides in the air set in motion by the water, than in the water set in motion by the mop. In fact, an amount of motion is caused in proportion to the motive force exerted; but after this motive force ceases to act on the particles set in motion by it, then those particles are sooner or later brought to rest by the vis inertiæ of matter, which thus resists motion in its progress as well as in its origin, and is, therefore, a property by virtue of which matter endeavours to be at rest, and not a property with a double action, at one time tending to keep matter at rest and at another time tending to keep it in motion.

Maxwell says that if the continuity of motion, as asserted by the 'first law,' is denied, and we suppose the existence of a law by which the velocity diminishes, and if such a reduction of velocity should be found to exist, that discovery might be interpreted not as a contradiction of

Newton's law, but as evidence of the resisting action of some medium in space.[1] Maxwell's arguments do not, however, either in the article from which I have quoted or in any other part of his book, meet the question now at issue. My argument is to the effect that the force of gravitation, extended from each material atom in the universe to every other atom, cannot be traversed by any body without expenditure of force to supply the work done in ceaselessly readjusting the equilibrium of gravitation disturbed by the moving body; and that, therefore, just as a bullet impelled by any initial velocity can traverse a piece of timber only for a distance proportioned to the initial force, and cannot continue for ever its motion through such timber without renewal of the motive force, so also a planet impelled through space cannot do the work of ceaselessly disturbing and readjusting the equilibrium of gravitation without just as ceaseless a renewal of the force which must of necessity be expended in doing that work. It shows a misconception of the question to argue, as Maxwell does, that it is only because there is a resisting medium in space that the momentum, if not renewed by fresh application of motive force, must ultimately be spent, and that the discovery of this resisting medium does not affect the so-called law of motion, because, 'supposing all this

[1] *Matter and Motion*, by J. Clerk Maxwell, Article xli.

done, we should have discovered not an error in the laws of motion, but a new fact in science.' [1] I say this shows a misconception of the question at issue, because the fact is that the force of gravitation which resists the motion is exerted by the planet just as truly as by any other particle of the universe, and therefore neither that planet nor any particle of the universe can have the asserted tendency to perpetuate motion given to it by a merely initial force. From a merely theoretical point of view the idea of a body having such an inherent tendency to continue in a state of motion, and the application of the term inertia, or vis inertiæ as preferred by Newton, to such a tendency, seems to me just as reasonable as the same arguments applied to the state of rest. But the existence of the force of gravitation extended throughout the universe excludes that law from the material universe; and I have shown that, as a matter of fact, the Earth does not move along its orbit by virtue of such a tendency, but is carried along by the action of an external force acting incessantly on it.

Playfair says that but for the discovery of the motion of the Earth that first law of motion might have remained unknown.[2] The ratio of forces and velocities which I have given above and more fully in previous works, shows that the so-called law is

[1] *Matter and Motion*, by J. Clerk Maxwell, Article xlviii.
[2] *Encyclopædia Britannica*, 8th edition, 4th Dissertation, p. 593.

no longer required for the purpose for which it was invented.

Robison, in reply to attempts to prove the truth of that first law of motion by the evidence of facts, argues that it is a mistake to suppose the law requires to be established on such a foundation; and tells us that it claims its position on higher grounds, being 'not a matter of experience,' but 'a necessary truth,' or 'a law of human thought.'[1] But though Robison says that the terms *inherent force* and *inertia* may be used for the purpose of 'abbreviating language,'[2] he makes them useless for the purpose for which the law was invented, for he says that by the First Law of Motion 'we suppose that the body continues in its former state of rest or motion unless we suppose that it is changed by some mechanical force;' and he says it is indifferent, as regards the sense of that law, 'whether those changes are derived from the nature of the thing or from external causes.'[3] But if the law allows us to suppose that the body by its own nature may be tending to change from a state of motion to a state of rest, then it does not serve to explain the continuous motion of the Earth. Newton's definition of vis inertiæ, on the other hand, gives a clearly defined and practical

[1] *Encyclopædia Britannica*, 8th edition, Article 'Dynamics,' p. 329.

[2] *Ibid.* p. 331.

[3] *Ibid.* p. 329.

meaning to that supposed First Law of Motion; and it certainly is reasonable enough for the mind to imagine the existence of matter under the conditions of that law as expounded by Newton. But it is just as certain that the mind can also reasonably imagine matter endowed with an inherent and constant tendency to motion only, or with a constant tendency to rest only; and the business of natural philosophy is not so much to ascertain by what laws matter might be governed, as to ascertain what are the laws by which it actually is governed; and this latter is certainly a question to be decided, not merely by an effort of the imagination, but by the practical evidence of physical phenomena.

Lord Kelvin and Professor Tait admit that 'the 'properties of matter *might* have been such as to 'render a totally different set of laws axiomatic.'[1] But though they say that the laws of motion 'must 'be considered as resting on convictions drawn 'from observation and experiment, *not* on intuitive 'perception,'[2] they are not more tolerant than Robison or Maxwell of opposition to the so-called first law of motion, but say that 'physical axioms 'are axiomatic to those only who have sufficient 'knowledge of the action of physical causes to enable 'them to see their truth.'[3] Among the familiar instances which are supposed by them to show

[1] *Natural Philosophy*, by Sir W. Thompson (now Lord Kelvin) and P. G. Tait, vol. i. part i., new edition (1879), p. 240.

[2] *Ibid.*

[3] *Ibid.*

opposition to the first law of motion to be due to ignorance of the action of physical causes, it is said that in railway travelling, 'if the motion of the 'train be checked by a sudden application of the 'brake, their inertia (which really maintains their 'motion) appears to urge the passengers forwards. 'A sudden starting of the train produces the opposite 'effect.'[1] As regards the latter point, there is no question at issue. It is agreed that the inertia of the passenger resists the impulse which gives him momentum with the train. And as regards the sudden application of the brake appearing to urge the passenger forwards, there also is no question as to the tendency of the passenger to continue his motion being due to the momentum imparted to him by the motion of the train. It is only because the momentum of the train is forcibly arrested by the action of the brake that the passenger's momentum, not being arrested by the direct action of the same force, tends to carry him onwards from his seat. The question at issue is merely as to whether the momentum is or is not inertia, and its asserted tendency to perpetuate itself. There is no question at issue as to the practical effects of the momentum in the above phenomena. Even the advocates of the First Law of Motion admit that the passenger's momentum will be expended before carrying him very far from the seat. It seems to me that in abandoning the position taken

[1] *Properties of Matter*, by P. G. Tait, London, 1894, p. 95.

by Robison and Maxwell, Lord Kelvin and Professor Tait have really given up the the only logical line of defence. At any rate, to say that my assertion that momentum is not inertia shows that I have not sufficient knowledge of the action of physical causes to enable me to understand their so-called axiom simply begs the question at issue. If Lord Kelvin and Professor Tait gave ever so perfect an explanation in theory of the motion of the hands of a clock under the supposition of the motive power being a spring, and I were to declare that I had examined that clock and found the motive power to be weights and that there was no spring in the clock, there would be no logic in an answer on their part to the effect that my statement showed me to be ignorant of the action of a spring. Even if they succeeded in showing me to be ignorant of the manner in which the motion might be caused by a spring, that would not alter the fact of the clock being moved by weights and not by a spring. My discovery of the fact of the above-mentioned clock being moved by weights would not be a proof that I could not understand the action of a spring; nor is my discovery of the fact of the Earth being moved along its orbit by the sun's gravitation a proof that I cannot understand the so-called Law which has been considered requisite to account for that motion.

It has been urged that the predictions of the positions of the heavenly bodies given in the

'Nautical Almanac' are based on the laws of motion and could not possibly be accurate unless those laws are true.[1] But, in the first place, I say it is the correctness of the laws of gravitation that enables those positions to be foretold. The First Law of Motion is a complement or counterpoise, which balances the action of gravitation. And in the second place I say that the fact of momentum being correctly treated in all those motions as a theoretical counterpoise to the forces of gravitation, is not evidence that momentum is inertia. The question as to whether the momentum of the Earth is sustained by the action of the sun's gravitation, as I say, or is merely a consequence of the Earth's inertia and not caused by the sun's gravitation, as is generally supposed, is quite independent of any possible question as to whether that momentum, whatever may be its cause, is correctly treated as a counterpoise to the sun's centripetal force of gravitation. In the first edition of my work published in 1866, I clearly pointed that out, saying: 'The effects are matters 'of observation, so that the point at issue is as to 'how those effects are caused; and I maintain that 'vis inertiæ holds the planets in equilibrium, the 'centripetal force of the sun's gravitation being a 'part of the planets' vis inertiæ just as much as the 'centrifugal force. . . . Vis inertiæ is the combined 'action of universal gravitation, which actually does

[1] *Dynamics*, by P. G. Tait, London, 1895, p. 3.

'keep the planet on the line of its orbit; opposing 'any tendency from as much as towards the sun.' [1]

Another phenomenon mentioned by Professor Tait as a proof of the so-called First Law of Motion is that a bullet dropped from the top of a tower 'does not fall vertically. It deviates to the least 'of the vertical, because it preserves while falling 'its superior eastward speed.' [2] As some points of this subject may become test questions for the rival theories of inertia, I will comment on it in some detail. I say that the direction the bullet takes on being let go from the top of a tower depends on the height of the tower as well as on its position, which may vary the course of the bullet in five different ways. Suppose the tower to be of infinite strength, and its position to be on the equator.

In the first place, let the tower be high enough to reach the orbit of the moon. I say that in such case the bullet would have to be fastened by a string, or secured in some manner, to keep it from moving from the tower. It would not rest quietly on the tower unless restrained. And I say that, if restrained by a string, the stress on the string would show the bullet to have a tendency to move, not towards the Earth, but off into space at a tangent to the top of the tower and in the opposite

[1] *The Ocean: its Tides and Currents, and their Causes*, 2nd edition, London, 1885, p. 88.

[2] *Properties of Matter*, by P. G. Tait, London, 1894, p. 97.

direction to the motion of the tower, because it would never get inherently into itself the momentum necessary to enable it to keep pace with the motion of the tower. In order to keep pace its momentum would have to be maintained by means of the string dragging it on with the motion of the tower. And if the string were suddenly severed, then, to a spectator on the tower, the bullet would be seen falling, not to the Earth, but backwards into space at a tangent to the motion of the tower; whilst at the same time, to a spectator looking only at the Earth and the bullet and ignoring the tower, the bullet, which would have been seen revolving in a circle round the Earth whilst held by the string, would, on the string being cut, be seen to fly off at a tangent from its former orbit in the direction of the motion, and with reduced velocity, exactly as a spectator sees a drop of water fly off from a mop in the direction of the twirl, though the drop of water in fact leaves the mop only because it does not adhere firmly enough to enable it to keep pace with the mop's revolving motion. The diminishing velocity of the bullet after being let go would allow it to fall back to the moon's orbit, in which it would then be revolved by the Earth's gravitation just as the moon is now revolved. According to the First Law of Motion there would be no diminution of that velocity in empty space, so that the bullet's momentum would tend to carry it away from the Earth *ad infinitum.* I say the momentum given by the dragging action of the tower would be gradually expended on the bullet being disconnected; and equilibrium of the Earth's direct and revolving forces would be established, as shown by Proposition XXV., book x., of 'The Ocean.'

In the second place, I say that some reduction of the height of the tower would change the direction in which the bullet would strain on the string. For the sake of argument, I will suppose the necessary reduction in the height of the tower to make it one-twentieth of its former height. Then, at that height the bullet would still have to be restrained in order to keep it from moving from the tower; but, instead of dragging back against the motion, the momentum imparted to it by the Earth's revolving force would make it strain on the string at a tangent onwards in the direction of the motion of the tower; and, if the string were cut, the bullet might either continue to rise as far as the orbit of the moon and come to rest on the surface of that body, just as a drop of water flung from a twirling mop comes to rest on the surface of the Earth; or it might perhaps find equilibrium in an intermediate orbit of its own. The reason why the bullet finds the motion of the tower too slow, and therefore leaves it and revolves alone with a faster motion, is given in Propositions X. and XI., book x., of 'The Ocean.'

In the third place, I say that some further reduction of the height of the tower, which, for the sake of argument, I will suppose to be one-thirtieth of its former height, the bullet, if free to move, would not *rise* from the tower, but would roll off in the direction of the motion and would continue to move faster than the tower, but with a motion gradually inclining downwards towards the Earth; though at this level a lighter substance than the bullet, such as vapour, might continue ceaselessly to revolve with a faster motion than that of the top of the tower. Some letters I received about seventeen years ago from a resident in Santiago, in latitude about 25° S., make it appear probable that there is such a belt of vapour revolving above the equatorial regions, and that it is the cause of the Zodiacal Light.

In the fourth place, let the height of the tower (still on the equator) be reduced to a practically possible height, so that the bullet would rest quietly on the top. I gather from the statement made by Professor Tait that he either knows from some practical experiment that a bullet dropped from the top of such a tower would fall east of the base of the tower, or else does not consider its position material to the question. All I can say is, that such would be the case as regards the higher tower, and that, if it is so as regards the lower tower, it certainly is not a proof that momentum is inertia. I have elsewhere [1] pointed out that on the surface

[1] *The New Principles of Natural Philosophy* (London, Longmans, 1885), p. 411.

of Jupiter the revolving force of gravitation seems to carry fluid matter eastwards in the equatorial regions faster than the solid surface, and suggested that the Earth's similar force might perhaps have some part in causing the well-known equatorial counter-currents; and the fact of the bullet falling east of the tower would accord with the idea that the same revolving force which perhaps carries the water eastwards has also some practical effect on the bullet whilst it is falling through the air, though insufficient to affect it whilst resting on the tower; or momentum might, in fact, be diminished without being so rapidly reduced as to prevent it from still being sufficient to carry the bullet faster than the slower motion at the base of the tower.

In the fifth place, if instead of being on the equator, the position of the tower be anywhere in either of the temperate zones, then I say the bullet falling from a tower of any practicable height has a tendency to fall south-eastwards in the northern and north-eastwards in the southern hemisphere; because the revolving force of the Earth's gravitation must *tend* to give it a faster motion than the surface of the Earth in those latitudes, and thus tend to carry it eastwards, in the same manner as the ocean water actually is carried eastwards; and the centrifugal action of that force tends to cause the bullet to be inclined towards the equator as it falls, because it acts at an angle crossing the vertical line in which the direct force of the Earth's gravitation acts. Both those actions are explained in Proposition XXVII., book x., of 'The Ocean.'

Experiments by Benzenberg from a church tower in Hamburg gave to falling bodies an average direction south-eastwards, but the experiments showed considerable discrepancies.[1] The idea of momentum being sustained by the inertia of matter is intrinsically at variance with the retarding and revolving actions of gravitation, which, according to my views, would determine the motion of the bullet in the experiment alluded to by Professor Tait as above.

Professor Tait seems to charge all translators of Newton's 'Principia' with having wrongly used the term *vis inertiæ* instead of translating it merely as *inertia*, and he also argues that the term *vis*, as used by Newton, does not mean force.[2] But the fact is that in his own English editions of works which were not, like the 'Principia,' published by him only in Latin, Newton himself uses the term *vis inertiæ* and never, as far as I have noticed, the term *inertia*. In the 'Treatise on 'Optics,' which was first published by him in English, and only subsequently translated into Latin, he says: 'It seems to me, further, that the 'particles have not only a *vis inertiæ*, accom-

[1] *History of the Theories of Attraction*, by J. Todhunter, M.A., F.R.S., 1873, ii. 454.

[2] *Dynamics*, by P. E. Tait, London, 1895, p. 353.

'panied with such passive laws of motion as 'naturally result from that force, but also that they 'are moved by certain active principles.'[1] This shows that it is Newton himself, and not merely translators of the 'Principia,' who is responsible for the use of the term vis inertiæ in English works. What Newton specially wanted in his definition of vis inertiæ, and in his first law of motion, was a force of motion, or momentum, tending to carry the Earth onwards in a straight line, in order to account for the Earth's resistance to the sun's gravitation; and, unless it can be shown that, as a cannon-ball emerges from the mouth of a cannon, the momentum tending to carry it onwards in a straight line and the Earth's gravitation, which bends it downwards from that line, cannot properly be called forces, it is useless to argue, as Robison and other philosophers have since done, that the idea of force as generally understood ought not to be connected with Newton's laws of motion and definition of vis inertiæ. The difference between the cannon-ball and the Earth as regards Newton's laws is that the momentum of the one is controlled by the Earth's gravitation, and that of the other by the sun's gravitation; but if the momentum of a cannon-ball as it smashes the side of a ship can, according to those laws, be called a force, then the momentum of the Earth is also a force. The effort

[1] Horsley's edition of Newton's *Works*, iv., 260.

of Robison and his followers to exclude the idea of force from the laws of motion is an episode in the rebellion of common sense against an illogical position; but the necessary reformation is to be attained, not in that direction, but only by the separation of all forces of momentum from the innate vis inertiæ of matter.

## *THIRD ESSAY*

### THE CARDIOID EARTH

Until recent years the expression 'as round as a bullet' was appropriately used to convey the idea of the object alluded to being perfectly round. Experience has, however, led to a change in the shape of those projectiles. Perfect roundness has not only ceased to be considered a necessary quality, but it is found that the velocity can be increased by a change from that shape, and the outer ring of the material of the round bullet at right angles to the proposed line of motion is now pushed towards the rear of the ball, so that the front is thereby made more or less pointed, and the rear approaches more or less a flat instead of a rounded form. I describe in the above manner the change in the bullet from its round towards a conical form, because that manner of describing the change of shape helps to illustrate the action by which the Earth's onward motion through space causes the ellipsoid shape or oblate spheroid, resulting from its axial rotation, to change slightly towards a cardioid form. I use the term cardioid as meaning heart-shaped, and, as the Earth's deviations from the ellipsoid form make the term more applicable to it than to the mathematical curve to which that term has been applied, it cannot reasonably be restricted to that technical sense.

I have recently found much more definite evidence of this change of shape than I supposed could be brought to bear on the question when, more than thirty years ago, I suggested that such a deviation from the ellipsoid existed. I did not then, and do not now, say that the Earth, like the bullet, has been so shaped for the purpose of facilitating its motion through space; but what I have pointed out is that, in consequence of its motion through space, it has a tendency to change from the ellipsoid to a cardioid shape.

The combined action of any motive and controlling forces which may tend to draw onwards in the central line of motion faster than in other parts of the Earth is resisted by the Earth's own gravitation, which endeavours to maintain it in a perfectly spherical form. And this resistance of the Earth's gravitation alone prevents the forces by which its orbital motion is determined from drawing its particles in the central line onwards and those outside that line relatively backwards, until they would at length be distributed in a ring of equal thickness along the line of the Earth's orbit.

Besides this resistance of the Earth's gravitation to the attempted elongation of its form, its axial rotation is incessantly changing the parts which, at any moment, lie in the central line of orbital motion, so that the effort of the orbital forces at any moment to change the Earth's shape in any direction is instantly counteracted by their action being rapidly moved from point to point as the Earth turns on its axis, and thus its gravitation, which would in any case prevent more than a slight modification from an ellipsoid shape, is able to prevent the orbital forces from having any effective action towards changing the Earth's shape in any direction.

This interference of the Earth's axial rotation with the tendency of the orbital motion to change its shape, is due to the latter motion inclining more to an east and west than to a north and south direction. Supposing the Earth, however, to have another motion in space, more directly across the plane of the equator, and not inclined so much from the line of the axis of rotation as to cause the direction to change between southwards and northwards as the Earth rotates; then the axial rotation would not interfere with the tendency to change of shape in connection with that motion in the manner I have described its interference with the orbital motion. It must also be observed that the manner in which the orbital forces tend to elongate the Earth along the line of its orbit does not exactly conform with that in which I have described the elongation of the bullet, for the backward drag, instead of being all round the parts remote from the central line of motion, has a direct action only on the portions of those parts which lie outside the line of the orbit. But as regards the suggested motion, more across the plane of the equator, the axial rotation, instead of neutralising the tendency to change of shape, would merely pass it continuously round the equatorial regions, equalising its action all round those regions without counteracting at one time the effect attempted at another time, and therefore those regions would have a relative tendency to lag in the suggested motion. The pressure of this lagging tendency would in the hemisphere in advance in the line of motion press the surface from latitudes of lesser to those of greater circumference, and thus tend to cause a subsidence of the surface; whilst in the opposite hemisphere the pressure would be from latitudes of greater to those of lesser circumference, and thus tend to cause a bulging out of the surface proportional to the subsidence in corresponding latitudes of the other hemisphere. Such a motion would thus tend to cause an elongation of the Earth's shape in the same manner as I have described the elongation of the bullet—not necessarily an absolute elongation, but a relative lengthening of the central line of motion as regards its proportion to the maximum measurement at right angles to that line.

I will now, before dealing with the evidence to which I have alluded as having recently been made available, sketch that of which I made use in 1866 as indicating a motion of the Earth southwards across the planes of the equator and the ecliptic.

In the diagram given in the frontispiece of this book the sphere shows the form which the sole action of the Earth's gravitation tends to cause; the ellipsoid, the change which axial rotation tends to cause; and the cardioid, the further change which onward motion tends to cause as just described. By a combination of those forms the configuration of the Earth would be such that if on its surface there lay water sufficient to cover one half of the surface, that water lying in each of the depressions and leaving the protuberances dry land, then the surface of the Earth would be divided into alternate zones of land and water; namely, land about the South Pole, water throughout the temperate regions of the southern hemisphere, a zone of land in the equatorial regions, a narrow zone of water north of the latter regions, a zone of land throughout the temperate regions of the northern hemisphere, and water about the North Pole.

The land in the temperate zone of the northern hemisphere, and that about the South Pole, would be raised by the action of the forces concomitant with motion through space, and the land in the equatorial zone by those concomitant with axial rotation. The relative positions of land and water, resulting as just described, strikingly correspond with the actual relative positions of land and water in the two hemispheres, as shown on the diagram, though the zones, instead of being continuous, are intersected by undulations running north and south. In the north there is the depression which contains the Arctic Ocean, surrounded by the continents of Europe, Asia, and North America; then there is the depression of which the Mediterranean and Caribbean Seas, separating Europe and North America from Africa and South America, form a part; and, thirdly, there is the continuous depression which contains the great Southern Ocean, separating South America, Africa, and Australia from the lands of the Antarctic regions.

Though when I first published the foregoing views I treated the tendency to a cardioid form as having influenced the actual distribution of land and water on the surface of the earth, I did not then suppose that any tendency of the present surface of the ocean to assume that form could be deduced from measurements already made. I find, however, that various discrepancies between actual measurements and what those measurements were expected to be under the supposition of the Earth being a true ellipsoid give practical evidence of the deviation from the ellipsoid towards the cardioid form, and I now proceed to deal with the evidence of those discrepancies in the following order:—

*First.*—Discrepancies between calculations for the length required to make a mètre equal the 10,000,000th part of the length of the meridian between the equator and the North Pole.

*Secondly.*—Discrepancies between the measured lengths of degrees of latitude and the theoretical lengths in different latitudes as computed under the supposition of the Earth being an ellipsoid.

*Thirdly.*—Discrepancies between the observed vibrations of the pendulum and the theoretical velocities of vibration in different latitudes as computed under the supposition of the Earth being an ellipsoid.

As regards the length of the mètre, I have pointed out that if, when fixing the length of that measure, with the intention of making it the 10,000,000th part of the length of the quadrant of the meridian between the equator and the North Pole, the distance from the equator to the pole was accurately estimated in a straight line, then the distance along the curve of the meridian must be greater than given by the calculation on which the length of the mètre was fixed if the shape of the Earth be cardioid instead of ellipsoid; for the length of the mètre was determined by the measured length of a degree in latitude 45° N. on the supposition of the meridian forming a regular ellipsoid, and, as the degrees of a cardioid meridian are shorter in that latitude than those of an ellipsoid, it is evident that, if the meridian is cardioid, the mètre, determined as above with the intention of 10,000,000 representing the distance from the equator to the pole, must be too short. If, on the other hand, the calculations had been based on measurements in higher latitudes, where the cardioid curve bends downwards to recross the ellipsoid, the mètre might have been made so much longer as to be more than the 10,000,000th part of the quadrant. If the calculation had been based on a measurement in an intermediate latitude, it might have given a length agreeing with the cardioid as well as with the ellipsoid form; but, the computation being based on an ellipsoid figure, it could not, except by mere accident, agree with the cardioid figure, and, as the cardioid gives a longer line than the ellipsoid from point to point, the correct length of the quadrant could have been given only by introducing an error into the estimate of the length of the polar radius or else into that of the equatorial radius of the earth. In Mr. J. H. Gore's work on Geodesy, published in New York in 1891, I find thirteen different estimates of the distance from the equator to the pole, all based on observations in which the meridian is assumed to be an ellipsoid. Of all the thirteen estimates above alluded to, that based on latitude 45° N., by which the length of the mètre is fixed, is the shortest. Six of them make the length of the quadrant more than a thousand mètres greater, the largest difference being nearly 2,000 mètres. Mr. Gore does not give the data for all those estimates, but only for the three to which he attaches most importance, and they all include more northerly measurements than that of the French arc, so that, if the meridian is cardioid, the discrepancies with the French arc are a natural consequence of the calculations being based on an ellipsoid. Those successive estimates all tend to show that it has been a mistake to suppose that the surface on which the various arcs were measured is an ellipsoid. I am not aware that it has ever been suggested that there can possibly be such an error in the French measurement of 1806 as to account for the discrepancies between the estimate based on it and those based on the more extensive measurements since made. If it be granted that there is not any sufficiently important error in the French measurement to account for the discrepancies, and that it cannot seriously be supposed that all the other twelve estimates at variance with it under the ellipsoid theory are incorrect, then the evidence given by the more recent estimates makes it reasonable to infer that the supposed ellipsoid is not the true meridian line, especially as a cause has been shown for a deviation from that ellipsoid to a cardioid form.

In one important instance a measurement at variance with the French estimate of 1806 has been 'corrected' into harmony, or approximate harmony, with the latter, and if this latter were not also at variance with all the twelve above alluded to, I would not venture to appear to disparage the *correction.* The story of this measurement and its correction is of special interest in connection with the present subject. An announcement to the effect that a pendulum which had been sent from Paris to South America was found to vibrate more slowly than in Paris led Sir Isaac Newton to assert in his 'Principia' that the Earth must be an oblate ellipsoid, being flattened at the poles by its diurnal rotation. This constituted the first discovery of the fact that, though the Earth is, roughly speaking, a sphere, it is not a perfect sphere but an oblate ellipsoid of rotation, making it an oblate spheroid instead of a true sphere. The French Government determined, after long-continued and keen discussions, to test this important statement by having an arc of the meridian measured in as northerly a position as practicable, and for this purpose despatched an expedition to Lapland in the year 1736, under the leadership of Maupertuis, and containing among their number the renowned mathematician Clairaut. The result of the measurement was to show not only the flattening required by Newton's ellipsoid, but even more flattening. That measurement, combined with the French arc which determined the length of the mètre in 1806, is not only incompatible with the perfect ellipsoid, but the discrepancy is such as also to show the cardioid deviation from the ellipsoid; the French arc being shortened as the cardioid curve rises, lifting the meridian above the ellipsoid level, and the Lapland arc being lengthened as the cardioid curve falls again and depresses the meridian below the ellipsoid level. An expedition, under Svanberg, despatched in 1801 to remeasure the Lapland arc, shortened the length given by Maupertuis; but I understand from the figures given by Sir John Herschel ('Outlines of Astronomy,' sec. 216) compared with those given by Galloway ('Encyclopædia Britannica,' article 'Figure of the Earth') that the Svanberg correction of the Lapland measurement still leaves the degree somewhat too long for the ellipsoid curve, though the remaining discrepancy is comparatively trifling. Mr. Gore (p. 169) says that Svanberg's line was measured only once, and that it is therefore impossible to form an opinion as to its accuracy; and in face of the twelve estimates to which I have alluded as having since been made, all giving a longer meridian than that based on the French arc of 1806, and also evidence given by pendulum observations to which I shall allude further on, I am inclined to think it not improbable that the work of Clairaut and Maupertuis has perhaps been too much corrected.

Before passing on to questions regarding the degrees of longitude, I will mention another 'discrepancy' in degrees of the meridian which resisted many determined attempts to 'correct' it into harmony with the supposed ellipsoid shape of the Earth, and still stands only partially corrected. Cassini was one of the principal, if not the principal of the numerous opponents of Newton's theory of the Earth being an *oblate* ellipse. He based his opposition on actual measurements which agreed with the theory of the shape being a *prolate* ellipse. And in consequence of the arguments on the subject, the French measurements were revised and extended, with the result that a meridian measured through France, on being divided into three sections, gave for the lengths of the degrees :—

| | |
|---|---|
| In the Southern Section . | 57,098 toises |
| In the Central Section . . | 57,060 ,, |
| And in the Northern Section | 56,960 ,, |

These measurements agree with the prolate ellipsoid, the degrees shortening in passing from the equator to the poles; and Cassini and his adherents did not disguise their surprise that any one should, in face of this further practical evidence to the contrary, which was merely a confirmation of previous evidence, persist in adhering to the arguments and evidence adduced by Newton, for the meridional degrees of his ellipsoid gradually increase in length in passing from the equator to the poles instead of decreasing, as shown by the above measurements. But fortunately Voltaire and Maupertuis had so clear a grasp of the broad principles of Newton's arguments that they continued to champion his views with such determination that the French Government at length sent the expedition to Lapland, to which I have already alluded as resulting in such overwhelming evidence of oblateness as to reduce the discrepancy in the French arc to the position of a question of detail. The Lapland arc proved too long to suit any of the French arc measurements, except by flattening the sphere at the poles. Maupertuis revised the French arc, with the result of reversing the chief part of the evidence against the oblate curve in that arc, leaving only slight evidence against it in one part of the arc. But this correction by Maupertuis, like the subsequent correction of his own work by Svanberg, above alluded to, is open to the obvious danger that the corrector would naturally be on his guard against the introduction of any errors in one direction—namely, the direction in which he supposed errors to exist—without being so specially on his guard against errors with an opposite tendency. The fact is that the shortening of the degrees northwards along the meridian in France, as shown in the detail of the French arc, affords evidence to the effect that the cardioid curve, after having further south been depressed below the ellipsoid level, is in that latitude either rising again to cross the line of the ellipse, or else is continuing to rise after having recrossed that line. Thus that discrepancy in relation to the ellipse harmonises with the cardioid curve, which, if it does not in any part of the meridian shorten the degrees northwards, must at least make the lengthening northwards less rapid than it would otherwise be. Further evidence to this effect and to the effect that the Lapland and French arcs are not really compatible with the ellipsoid level will appear as I now pass on to some discrepancies in the lengths of degrees measured along parallels of latitude.

Special attention was directed to the meridian measurements by the discussions in connection with the calculations by which the length of the mètre was fixed in 1806. The question as to the length of degrees along parallels of latitude has not received so much attention. But these latter better suit my purpose because in some parts of the meridian it is difficult to decide theoretically whether the change to the cardioid should increase or decrease the ellipsoid estimate.

Thomas Galloway, in his treatise on the figure of the Earth,[1] gives the following table showing differences between the lengths of degrees ascertained by actual measurement along the following parallels of latitude and the respective lengths

[1] *Encyclopædia Britannica*, ix. 564, 8th edition.

as computed on the supposition of the Earth's figure being a true ellipsoid:

| Latitude | Measured Degree | Computed Degree | Difference |
|---|---|---|---|
| 43° 31′ 50″ | 266,345 | 265,154 | + 1,191 feet |
| 50° 44′ 24″ | 232,331 | 231,542 | + 789 " |
| 50° 44′ 24″ | 231,579 | 231,542 | + 37 " |
| 45° 43′ 12″ | 255,480 | 255,370 | + 110 " |

It will be noticed that the actual measurements show the degrees along the above-mentioned parallels to be longer than computed on the supposition of the Earth being an ellipsoid. The evidence of those measurements is, in fact, to the effect that the surface of the Earth at the ocean level in those latitudes is raised above the ellipsoid level; and they therefore confirm the measurements already alluded to as showing a corresponding discrepancy between the ellipsoid and the measured length of degrees along the meridian.

Galloway, in commenting on the above discrepancies, says: 'It is remarkable that all these 'errors are affected with the same sign; and the 'circumstance might seem to strengthen the con- 'clusion indicated by a comparison of the errors of 'the meridional arcs—namely, that the meridional 'curve is not exactly an ellipse, but protuberant 'between the latitudes of 40° and 52°; in conse- 'quence of which the degrees of meridian are 'shorter, and the degrees of parallel longer, between 'those latitudes than if the Earth were a regular 'ellipsoid.'

The foregoing makes it, I think, evident that the measured lengths of degrees, whether measured along the meridian or along the parallels of latitude, show that the surface of the Earth in the northern hemisphere deviates from the ellipsoid in accordance with the suggested tendency to assume a cardioid form.

La Caille, in the years 1750 to 1752, made measurements at the Cape of Good Hope for the purpose of determining the length of a degree of the meridian there, and found it longer than accepted measurements in corresponding latitudes of the northern hemisphere.[1] That discrepancy agrees with the cardioid figure, for, as the bulging of the spheroid in the northern hemisphere shortens the degrees, the subsidence in corresponding latitudes south lengthens them. La Caille supposed he had discovered that, as a matter of fact, the hemispheres are not symmetrical, but measurements made in 1848 by Maclean gave an opposite discrepancy, thus making it out of place to base any argument on La Caille's work.

I now pass on to the evidence given by the vibrations of the pendulum.

The discrepancies between the observed vibrations of the pendulum in different latitudes, and those computed on the supposition of the Earth

[1] *Geodesy*, by J. H. Gore, New York, 1891, p. 118.

being a true ellipsoid, do not in many places appear yet explicable; but the important feature of the bulging of the ellipsoid in the northern hemisphere, between latitudes 40° and 52° N., changing the ellipsoid to a cardioid, is indicated by the average of the observations given by Galloway (page 574) in those latitudes, which give the increased or decreased rate of observed vibrations as compared with the computed vibrations in twenty-two observations as follows :—

| | | |
|---|---|---|
| − 1·16 | − 1·12 | − 0·74 |
| − 1·32 | + 0·09 | − 0·74 |
| − 2·53 | + 0·14 | − 0·74 |
| − 2·24 | − 0·98 | − 0·74 |
| − 1·98 | − 0·84 | − 0·74 |
| − 0·92 | − 1·03 | − 0·75 |
| − 1·68 | − 0·74 | |
| − 1·34 | − 0·74 | Average − 1·05 |

These vibrations of the pendulum corroborate the measurements in those latitudes, as the cardioid form gives slower vibrations there than the ellipsoid form, because, the surface being raised above the supposed level in those latitudes, the force of gravitation at the ocean level is less, and the vibrations of the pendulum consequently slower than the estimate. The most northerly observation given by Galloway is in Spitzbergen, 79° N., where the observed as compared with computed vibration was + 3·70, showing a quicker vibration than computed for the ellipsoid. This quicker vibration corroborates the measurement by Maupertuis in

Lapland, which flattens the ellipsoid there, and also agrees with the cardioid form, which, if giving a slower vibration in the temperate zone, would give a quicker vibration than the ellipsoid in high latitudes, because the cardioid raises the level in the temperate zone above, and depresses it in higher latitudes below the ellipsoid level.

Galloway does not give any pendulum observations taken by Maupertuis, but Mr. Gore states that the latter records that the so-called discrepancy in his measurement of the arc of the meridian in Lapland was corroborated by pendulum observations, and I have not found it anywhere stated that those observations were corrected by Svanberg when, as above pointed out, he corrected the meridian measurement.

The above evidence given by the vibrations of the pendulum seems clearly to corroborate that given by the measurement of degrees of latitude and longitude with which I have dealt, and suffice, I think, to show that a deviation from the ellipsoid to the cardioid does actually exist. I have elsewhere given other measurements to the same effect; but in many cases complex considerations are involved, and in many others the discrepancies as regards the ellipsoid, both in the measurements of degrees and in the vibrations of the pendulum, appear inexplicable, even with the substitution of the cardioid curve, which seems as yet to give merely a step which clearly solves some of the discrepancies but leaves many others still unexplained.

The stitching round a cricket-ball affects the relative lengths of its diameters more than the relative lengths of the Earth's diameters is affected by its axial rotation, though this difference, caused by the change from the sphere to the ellipsoid, is about twenty-six miles; and the difference of the ocean level in the two hemispheres, caused by the change from the ellipsoid to the cardioid, is perhaps a quarter of a mile in the temperate zones, and one or at most two miles in the polar regions. Thus, though in general terms it is sufficiently correct to call the Earth a sphere, which is, more accurately speaking, an oblate spheroid, that spheroid is not a true ellipsoid of rotation, but deviates from that towards a cardioid form.

## *FOURTH ESSAY*

### TERRESTRIAL MAGNETISM

I VENTURE to give some reasons for considering that a paper read at a meeting of the Royal Geographical Society, March 17, 1897, by Mr. A. E. Reeves, places the science of terrestrial magnetism on a new basis, and constitutes the greatest advance in our knowledge of the subject that has been made in recent times. The new position to which I allude has been created by the arguments by which Mr. Reeves makes it appear evident that it is a mistake to suppose, as all authorities have hitherto, either expressly or tacitly, done, that the Earth is magnetised in the direction of the north and south magnetic poles at an angle, or at various angles, crossing the axis of rotation. His arguments seem to show that the axis of rotation is also the magnetic axis; and this now, for the first time, clears away some difficulties which baffled Edmond Halley two hundred years ago.

Halley, in papers presented to the Royal Society in the years 1683 and 1695, gives lists of variations of the compass then recorded in different parts of the world, and from those observations he arrives at the conclusion that they indicate the action of at least four magnetic poles, two situated near the North Pole and two near the South Pole.

Mr. Reeves points out that if the Earth is magnetised in the line of the axis of rotation, the angle which the vertical needle at the magnetic pole makes with a line parallel with the Earth's axis is a reason for the north pole of the needle to point southwards in positions between that and the geographical pole, as it is known to do to the north of the North American magnetic pole.

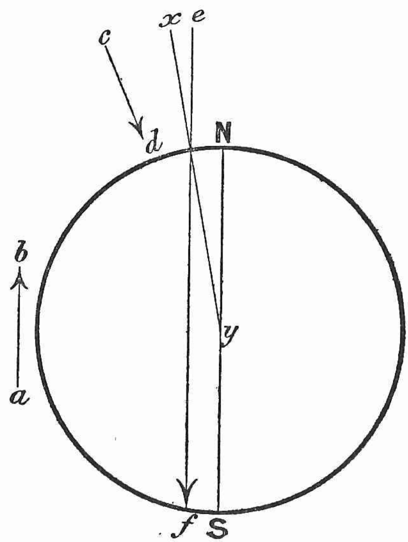


Fig. 2.—The Magnetic Needle.

Let *a b* represent the direction in which the north pole of the needle points at the equator, and

$c\ d$ the vertical needle at the so-called north magnetic pole. Between the latter and the geographical pole the area is entered in which the magnetic lines are parallel, and therefore the horizontal component of the direction in which the north pole of the needle points in the line $e\ f$ is south, as it must point from north to south across the vertical line $x\ y$.

If it be admitted that the magnetic axis is not merely a mathematical line, but that the north and south magnetic poles must each represent an extensive area of the Earth's surface, the phenomena above indicated seem to be almost necessary consequences.

The above argument, which, notwithstanding its manifest simplicity, is, as far as I know, now first invented by Mr. Reeves, is almost obviously applicable to every meridian, and shows that if the Earth's magnetic force were equal on all meridians then the position of a dipping needle would be vertical all along a parallel of latitude immediately outside each of the polar areas, and also at the north and south poles of the Earth's axis of rotation. And the horizontal direction of the needle in both of the polar areas, wherever sufficient deviation from the vertical to show any horizontal direction existed, would give the north pole of the needle a southerly direction all over both those areas.

As, however, the meridians and portions of meridians which traverse the oceanic areas are weaker magnetically than those which traverse the intermediate continental areas, the Earth presents the phenomenon of a flattened magnet, the shoulders of which tend to form the four poles required by Halley. But for this flattening of the magnet the needle would not become vertical on any one meridian sooner than on any other as the polar regions are approached. And after crossing the circle, or parallel of latitude, in which the needle became vertical, the north pole of the needle would, being then over one *end* of the great terrestrial magnet, turn southwards, instead of northwards as it does all round the *sides* of that great magnet by which it is controlled; and on reaching either pole the needle would again become vertical. It is only the flattening of the magnet that creates, before the true poles are reached, superficial magnetic poles over each shoulder of the magnet.

The two northern poles, one in America and the other in Asia, have been discovered since Halley's time; the former by the expeditions under command of Sir William Parry in 1819 and Sir John Ross in 1831, and the latter by Professor Hansteen in an overland journey through Siberia in 1827. One of the south poles—namely, that on the Asiatic side, south of Australia—was also approached in 1841 by the expedition under command of Sir James Ross, leaving only the

South American pole to be discovered to complete the four poles required by Halley.

Halley's opinion that *at least* four poles are requisite to account for the variations of the needle may not improbably be justified by its being found that the African continent gives prominence to a third Antarctic magnetic shoulder, making in all, for practical purposes, five magnetic poles.

Halley was perplexed at finding that at least four poles were required to explain the variations of the compass, and to account for them suggests that there may be an interior sphere within the Earth with different magnetic poles and a different period of rotation, and perhaps again a smaller sphere within that interior sphere; but Mr. Reeves's paper now, for the first time in history as far as I know, gives a clue which shows that Halley's poles can be accounted for on the supposition of the Earth being only one magnet, of which the magnetic axis is the same as the axis of rotation.

Mr. Reeves's paper contains points on which I have no reason to comment, as my object is to submit a few observations, partly according with and partly in modification of Halley's views regarding the variations of the needle, as the removal of the above difficulty by which he was perplexed opens new questions for discussion on the new basis on which the subject has been placed.

First, as regards the diurnal variation. I have elsewhere long ago pointed out, without then supposing the matter to be connected with the present question, that a revolving force within the Earth tends to make the equatorial regions rotate in about eighty minutes, but the extra-tropical and polar regions form a drag retarding the motion of the outer crust of the Earth and preventing it from keeping pace with a fast rotating fluid interior. Also, that the sun's gravitation is not only the *controlling* force in the Earth's orbital motion, but also the *motive* force, and that it tends to raise a tide on the part of the Earth's surface which may from time to time be in advance in the line of the orbital motion.

Part of the outer crust of the Earth is over the position of the interior tide every morning, and then passes on eastwards, leaving the tide in the same position in relation to the centres of the Earth and the sun, thus giving a cause for a diurnal variation of the declination of the needle if the fast rotating interior is the principal source of the Earth's magnetic force.

The facts that in the early morning in the northern hemisphere the north pole, and in the southern hemisphere the south pole of the needle turns *eastwards*, and during the forenoon and midday in the northern hemisphere the north pole, and in the southern hemisphere the south pole of the needle turns *westwards*, accord with the suggested position of that tidal action.

Secondly, as regards the annual variation. Every year the north pole falls towards the position of the tide just described from the vernal to the autumnal equinox, and then recedes from it, so that the tidal action is in its most northerly position at the time of the autumnal equinox, and at its most southerly position at the time of the vernal equinox. Thus this annual swaying of the tidal action between the northern and southern hemispheres gives a cause for an annual variation by a more complicated action than that which causes the diurnal phenomena to which I have above alluded. The action is merely a complication of precisely the same action as that of the diurnal variation, being due to a movement of the diurnal morning tide northwards and southwards once in each revolution of the Earth round the sun.

And, thirdly, as regards the secular variation. Though, for the slight difference between the periods of rotation of the outer and interior parts of the Earth which Halley suggested to account for the secular variation in the declination of the needle, I have substituted a much greater difference of velocity, changes of the bearing of the deep-seated force of magnetism in relation to the surface of the Earth may nevertheless account for the secular changes as suggested by Halley; for the secular variation may be the effect of a tidal action precisely similar to that which I have suggested as the cause of the annual variation.

Just as the north and the south poles alternately sway towards and from the direction of the Earth's orbital motion, so also they may be slowly swaying in another line of motion caused by a force which completes a cycle in a long series of years.

In such case the secular variation and the annual variation are both due to derangements of the diurnal tide caused by the action of the forces which draw the Earth onwards and make it sway in its lines of progress through space. It is true that some eminent authorities have supposed this secular variation to be in some manner more directly due to the sun's action; but that was not the opinion of Halley nor of Airy, nor of many other students, who have argued that magnetic action proceeds directly from far below the Earth's surface. Captain Craik, in his article on the subject, published with the 'Challenger' Reports, dealing with the most recent investigations, gives a decided opinion to the effect that this secular variation is chiefly due to continuous redistribution of magnetical matter in the interior of the Earth.

An objection which may on first thought be suggested as regards the tidal influence on the needle is that, as the moon's tidal action on the ocean is greater than that of the sun, variations at lunar intervals ought to be more marked than those which follow the diurnal and annual changes of the sun's position in relation to any part of the

Earth's surface; whereas, in fact, though attempts have been made to connect the moon's influence with magnetic changes, it has never been accepted as an important factor. Professor Sylvanus Thompson, now a leading authority, alludes to variations which follow the changes of the moon's position as comparatively infinitesimal, the diurnal as well as the annual variations being chiefly dependent on the position of the sun.

Against the above objection to the effect that, in any tidal action, the moon's influence should be more effective than the sun's, it may, however, be urged, in the first place, that the solar tide in question is the only tide raised by any force yet discovered which, as far as practical purposes are concerned, has an effective action in carrying the Earth along through space; and, secondly, a magnetic action similar to that which I now connect with the sun's action I long ago suggested to be connected with a motion of the Earth southwards through space, causing the configuration of the Earth which forms the depression in which the great southern ocean rests, whilst the mean level of the northern hemisphere in corresponding latitudes is elevated. And I then also suggested that the same action must tend to depress the Arctic and to elevate the Antarctic regions.

Though a good deal has in the meantime been written against the probability of the relative distribution of land and water in the Arctic and

Antarctic regions being as above suggested, those views have now, I think, been fully justified; first, by the results of the expedition under Sir George Nares, and more recently by that under Dr. Nansen. A slow change in the direction of the motion which causes that configuration of the Earth, and of the variation of the tidal action dependent on the force by which that motion is caused, supply a cause for the secular variation of the declination and inclination of the needle. The chief factor in causing the secular variation may, in such case, remain unknown until the cause of the motion southwards and its connection with the paramount force of magnetism within the Earth has been discovered. But, according to the foregoing suggestions, the diurnal and annual variations of the needle are due to tidal disturbances of that magnetic force within the Earth by the sun's action on it as the Earth rotates on its axis and revolves round the sun.

The views just above alluded to as connecting the configuration of the Earth with magnetic action are merely extended by the arguments of this paper, which have been rendered possible by Mr. Reeves's arguments to the effect that the magnetic poles of the Earth's surface are superficial accidents which are not at variance with the supposition that the Earth, in fact, rotates on its magnetic axis.

# *FIFTH ESSAY*

## THE SPINNING-TOP

In two public lectures which I delivered in the year 1877, and which now form Chapters VI. and VII. of my work on 'The New Principles of Natural Philosophy,' I explained how the combined action of astral gravitation resists the orbital motions of the planets and forms the centrifugal force which keeps them from falling towards the sun; and at the same time I pointed out that a similar action of gravitation is the force which supports the spinning-top, preventing it from falling on its side whilst rapidly rotating.

I will illustrate the position in which the discussion of the theory of the spinning-top now stands by one of many interesting points in a lecture delivered by Professor John Perry in 1890, and published under the title 'Spinning-Tops.' The term 'precession' is applied by Professor Perry to the revolving motion of the top when rotating in an inclined position.

Professor Perry says: 'Rapid rising to the upright position is the invariable sign of rapid rotation in a top. . . . All so well known as this rising tendency of a top has been ever since tops were first spun, I question if any person in this hall knows the explanation, and I question its being known to more than a few persons anywhere. Any great mathematician will tell you that the explanation is surely to be found published in 'Routh,' or that at all events he knows men at Cambridge who surely know it, although he has now forgotten

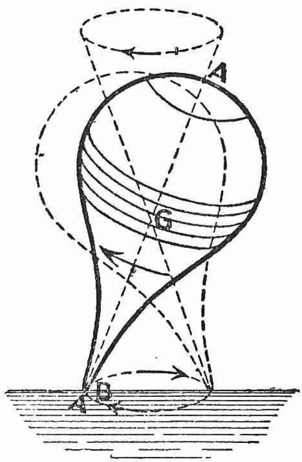


Fig. 3.

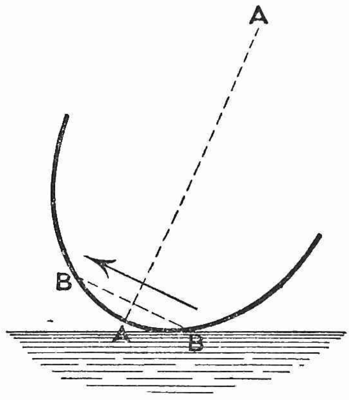


Fig. 4.

The Spinning-Top. From Professor John Perry's Diagrams 32 and 33.

those elaborate mathematical demonstrations which he once exercised his mind upon. I believe that all such statements are made in error, but I cannot be sure. A partial theory of the phenomenon was given by Mr. Archibald Smith in the 'Cambridge Mathematical Journal' many years ago, but the problem was solved by Sir William Thomson and

Professor Blackburn when they stayed together one year at the seaside reading for the great Cambridge mathematical examination.' . . . 'The simple reason . . . Thomson gave me sixteen years ago.' . . . 'As I am not touching the top, and as the body does rise, we look at once for something that is hurrying on the precession, and we naturally look to the way in which its peg is rubbing on the table, for, with the exception of the atmosphere, this top is touching nothing else than the table. . . . You will observe that the peg (fig. 3) is rolling round a circular path on the table, G being nearly motionless, and the axis A G A describing nearly a cone in space whose vertex is G, above the table. Fig. 4 shows the peg enlarged, and it is evident that the point, B, touching the table is really like the bottom of a wheel, B, B, and as this wheel is rotating the rotation causes it to roll *into* the paper, away from us. But observe that its mere precession is making it roll *into* the paper, and that the spin, if great enough, wants to roll the top faster than the precession lets it roll, so that it hurries on the precession, and therefore the top rises. That is the simple explanation: the spin, so long as it is great enough, is always hurrying on the precession.'

In a footnote to part of the foregoing Professor Perry says: 'When this lecture containing the above statement was in the hands of the printers, I was directed by Professor Fitzgerald to the late

Professor Jellett's 'Treatise on the Theory of Friction,' published in 1872, and there at page 181 I found the mathematical explanation of the rising of a top.'

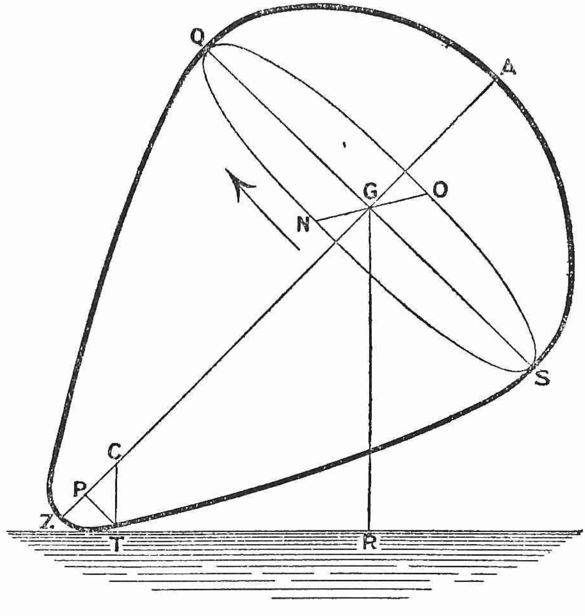


FIG. 5.—THE SPINNING-TOP. FROM PART OF DIAGRAM 28 IN PROFESSOR JOHN H. JELLETT'S 'THEORY OF FRICTION.'

A, Q, Z, S, Meridional Section; Q, N, S, O, Equatorial Section; G, Centre of Gravity; A, Z, Axis; Q, S, Equatorial Diameter; N, O, Equatorial Diameter, perpendicular to Q, S; T, R, Horizontal Plane on which the top rests; the line N, O, and the plane Q, N, S, O, are perpendicular to the plane of the paper, and the other lines all in that plane; T, the point at which the lines C, T, and P, T meet, is also the point of contact with the ground.

I will now turn to the work of Professor Jellett, where the action of the friction alluded to as above by Professor Perry as the cause of the top rising to an upright position is treated in a detailed and clearly defined manner.

Professor Jellett says: 'We shall assume, for the purpose of simplifying the question, that the motion of rotation round the axis of figure is so rapid, as compared with either of the other angular motions of the body, that in determining the direction of the force of friction the motion of T (fig. 5), considered as a point in the body, may be taken to be perpendicular to the plane of the paper. If, therefore, the rotation be in the direction of the arrow at N, the force of friction is parallel to the line N, O, the velocity of rotation being so great that there is necessarily slipping at T.'

As regards the above, I say, first, if the equatorial diameter, Q S, were perpendicular to the horizontal plane, T R, then the friction at the point of contact (which would then be S instead of T) would be perpendicular to *the plane of the paper*. And that is the only position in which the friction, if it can be said to be perpendicular to *the plane on which the top rests* in any position, can be so.

Secondly, if the axis of rotation, A Z, were perpendicular to the horizontal plane, T R, on which the top rests, then the friction at the point of contact (which would be Z) would be not only perpendicular to the plane of the paper, but also *in* that plane, and at every intermediate angle; and at the same time it would be entirely in the horizontal plane on which the top rests, and not at any angle to that plane.

In the latter position (upright), the 'motion of T' is parallel to the horizontal plane on which the top

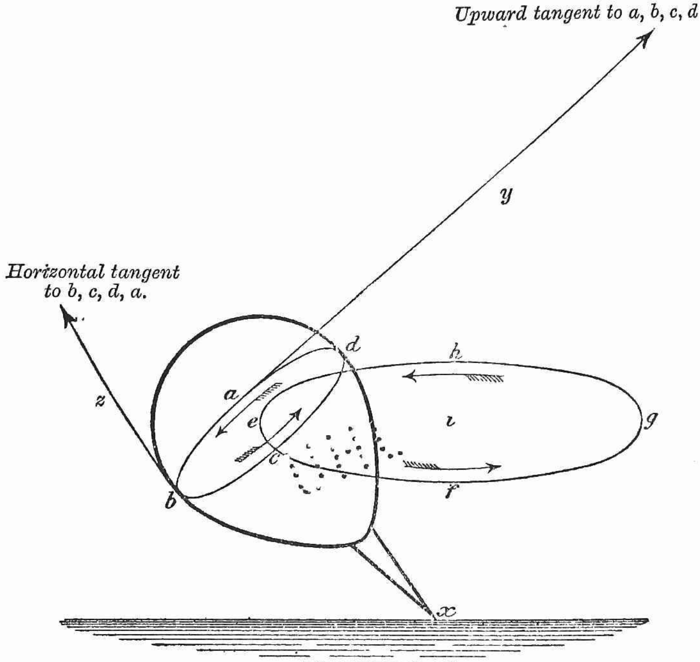


FIG. 6.—THE SPINNING-TOP.

*a, y* is the primary force of resistance which keeps the top from falling, and makes it *revolve* in the direction *a, e, c*; *b, z* is the secondary force which resists the revolving motion *e, f, g, h*, and *lifts* the top in the direction *b, e, d*. Each of those motions moves the centre of gravity, *e*, in an effort to equalise the combined velocities of rotation and revolution in all parts of the top.

rests, and becomes perpendicular to that plane only when the top has fallen through 90°, making the plane of its rotation perpendicular to the horizontal plane on which it rests.

I do not doubt the correctness of Professor Jellett's mathematics, but I am not sure that the conclusion he arrives at, to the effect that the friction at the point of contact with the plane on which the top rests tends to cause it to rise to an upright position, is not vitiated by the transition of the data on which the argument is based from the plane of the paper to that on which the top rests. Be that however as it may, the fact is that by a simple mechanical arrangement the top can be spun without any such friction, and if then given a motion of revolution similar to that of the common spinning-top, it will also rise, clearly showing that there is some other force than the friction at T, which tends to lift it into an upright position. It seems to me that Professor Jellett's argument is intrinsically correct, and that the resisting action of friction which he applies to the point of contact with the ground is identical with the resisting action of gravitation which I apply to every particle of the top. He, in fact, deals with merely a fraction of the resisting action of gravitation.

I will first show the manner in which astral gravitation brings the common spinning-top to an upright position, and then show the same action on a top not supported on its peg but suspended from opposite points of an equatorial diameter. The effect of the demonstration I am about to give will be to show that it is an excess of centrifugal force on the side S (fig. 5), over that on the side Q, acting with the point of the top's contact with the ground, as a pivot for leverage, that lifts the top, because it endeavours to bend the plane of rotation on the side S (fig. 5), away from that point and on the side Q towards it.

The following explanation of the manner in which astral gravitation supports the common spinning-top and brings it to an upright position is given in Proposition XXV., book x., of 'The Ocean.'

Suppose the velocity of rotation of the top shown in fig. 6 to be expressed by any figure, say 8, in the direction *a b c d*, and the resisting force of astral gravitation to be as the square of the velocity acting in the same manner as I have shown, that it supports the Earth against the sun's gravitation, then the resistance of astral gravitation is as the square of 8 on each side; but immediately the top moves downwards from that position, say with the velocity of 4 in a downward direction, then the particles on the side *d a b*, with this motion added to them, have their velocity increased; and that velocity of 4 added to the velocity of 8 makes the velocity of 12, whereas the velocity on the side *b c d* is reduced to 4; so that the resistance of astral gravitation on one side is 16, the square of 4, whereas the force on the other side is 144, the square of 12; so that the force of astral gravitation on the side *d a b* is 144, drawing upwards, and on the side *b c d*, 16, drawing downwards. Thus there is a difference of 128, representing an excess in the force of astral gravitation drawing upwards; and unless the Earth's power of gravitation drawing downwards exceeds that difference of 128 the top cannot fall; it is then supported, and it is only when the top's rotation becomes so slow that the Earth's power of gravitation dragging it down is greater than the difference between those two forces of astral gravitation that the top can fall.

The tangential action of the force at *a*, which supports the top (constantly lifting that side whilst the side, *c*, falls) carries the top round with the motion of revolution in the direction *e f g h*. A force of astral gravitation at the point *b* then resists this horizontal motion exactly as that at the point *a* resists the downward motion; and the tangential action of the force at *b* also supports the top, because it tends to carry the point *b* farther than the point *d* from the centre *x*, just as the tangential action at the point *a* makes the top revolve by tending to carry the point *a* farther than the point *c* from the centre *x*. If the velocity of rotation be great, the force at *b* will lift the top into an upright position, in the same manner as the force at *a* carries the top round in the direction *e f g h*.

To show that it is the centrifugal force in the plane of rotation, and not the friction at the point of contact with the ground, that lifts the top to its upright position, take, instead of the common top of figure 6, a gyroscopic top arranged as in figure 7, in which the top may be supported either on a bar passing through a hollow axis, or else, in order to reduce the friction, it may hang on pivots inserted into slight hollows at each end of the axis of rotation. If, then, the top be evenly balanced on the gyroscope, it will not have

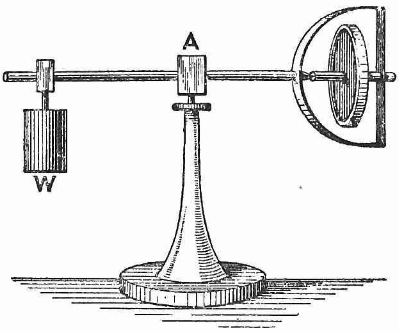


FIG. 7.—A GYROSCOPIC TOP.

The disk on the right-hand side is the top, which rotates on pivots at each end of its axis. In the block A a pivot allows the top to move vertically; and the supporting rod which rests in the hollow stand allows the top to revolve.

any motion of precession unless some force is specially applied to give it that motion; and if the direction of that precession be given in the same direction as that of the common top, then the gyroscopic top will rise and bring its axis to as nearly a vertical position as the mechanism will allow. The action of the centrifugal force is there precisely the same as in the common top, and the same rising motion occurs, though the friction to which Jellett and Perry attribute the rising of the top does not exist.

Further evidence that the centrifugal force is correctly treated as the cause of the rising of the common top is to be obtained from the above mechanism by reversing the direction of the precession. The excess of centrifugal force will then be on the upper side of the top and will force the top downwards instead of upwards, and by the motion in that direction will bring the axis to as nearly a vertical position as the mechanism will admit.

If the foregoing were not sufficient to show that the friction at the point of contact with the ground is not the force which lifts the top, other experiments, as I now pass to another point at issue which requires attention, will incidentally support that evidence.

That the precession, or revolving motion, of the common top when spinning in an inclined position is not dependent on the rolling of its peg, like a wheel, on the ground may be shown by the same gyroscopic top shown in fig. 7 (p. 106). In the first place, whilst the top is spinning balanced as in that figure, remove the balance weight, w, so that the Earth's gravitation may tend to pull the top down just as it tends to pull down the common top. It will then be seen that the gyroscopic top, instead of falling, will instantly have the same revolving motion as the common top. That is to say, its precession will be in the same direction as that of the common top. But if, instead of removing the balance weight, an extra weight be added so that the Earth's gravitation, by tending to pull the weights down, tends to make the top rise instead of fall, then there will instantly be a motion of precession, but in the opposite direction to that of the common top. In each case the precession agrees with the excess of centrifugal force acting from the pivot, A (fig. 7), on which the top is supported. There is no rolling motion at the pivot, or at the point of the supporting rod, which is inside the stand and is always vertical. There is no rolling motion with the onward motion of precession. It is thus evident that the precession is dependent on the centrifugal force and not on any rolling of the peg along the ground.

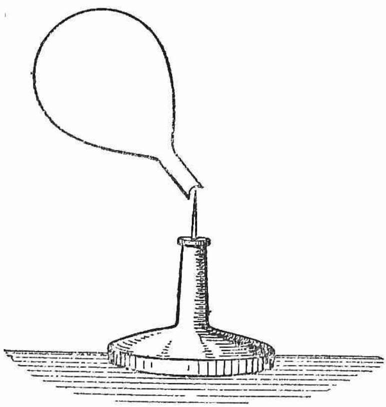

FIG. 8.

Further evidence that neither the friction nor the rolling of the peg is the cause of the top rising can be obtained by making the peg of the top slightly concave, instead of pointed, and then placing it whilst rotating in an inclined position on a sharp-pointed support, as in fig. 8. By that arrangement there is neither the rolling motion on the ground required by the Kelvin-Perry part of Professor Perry's argument nor the friction required by the Jellett-Perry theory, for the friction of the top against its point of support is reversed whilst the top is in the inclined position; but notwithstanding that reversal of the friction, the top so arranged revolves just as a common top does, and as it revolves it gradually rises to an upright position just as a common top does.

Professor Perry has, I presume, ascertained by experiments that a top with a smoothly rounded peg rises to an upright position more quickly than with a pointed peg, so that the rapidity of rising appears in the common top to be proportioned to the diameter of the peg at the point of support, or, in other words, to the *diameter of the wheel* on which it revolves, and Professor Perry has therefore perhaps accepted that as evidence that it is the rolling of the peg on the ground that causes the top to rise. The experiments I have shown have made it, however, clear that the top will rise without any such rolling motion, and careful observations of the motion of a common top make it, I think, clear that the reason why a top with a sharp-pointed peg rises more slowly than with a smoothly rounded peg is that the pointed peg resists the slipping which the force of gravitation tending to draw the top down tends to cause;

whereas when the top is spun on the rounded peg the peg more readily yields to the slipping motion, allowing the top to yield more directly to the downward force of gravitation, and the resisting force of gravitation, increasing as the squares of the resulting velocities, in the tangents *a* and *b*, fig. 6 (p. 102), consequently lifts the top more rapidly. With a ball instead of a point for a peg the rise of the spinning top to an upright position seems to be as quick as its fall to a prostrate position would be if not spinning. For the centre of gravity, G (fig. 5, p. 100), to fall in the straight line G R, the point of support, T, must slip away farther from the point R; and as it does so the resistance of astral gravitation at the point O gives the top a horizontal twist, instantly followed by a vertical twist given by the resistance of astral gravitation at the point S, and the combination of these actions brings the top by a spiral motion to the upright position.

Perhaps some who have followed the argument by which I have shown that centrifugal force, and not either the friction or the rolling of the peg on the ground, causes the revolving motion in the horizontal plane, and also that in the vertical plane which lifts the top to an upright position, may, whilst agreeing as to those motions being caused by the centrifugal force, be disposed to consider the direct action of the motion, and not the force which resists the motion, to be the centrifugal force.

The view taken by Edward John Routh in his 'Dynamics of a System of Rigid Bodies,' published in 1860, is evidently to the effect that the direct action of the motion controls all the movements of the top; but his mathematical demonstrations may, I think, be regarded rather as explanations of what the relative velocities of motion, in different positions and movements of the top, must be round the three axes—namely, the axis of rotation; that of horizontal revolution, to which Perry has applied the term 'precession;' and that of vertical revolution, to which Perry has applied the term 'nutation.' Routh's demonstrations can scarcely be considered as explanations of the cause of those respective movements, whereas Professor Perry's work is an attempt to solve the causes. I do not doubt the correctness of Professor Routh's demonstrations, and, if correct, an analysis of them must of course show that a combination of the rotating and horizontal velocities calulated for a top under the conditions shown in figure 6 (p. 102) gives the point *d* less velocity than the point *b*; but there does not appear to be anything in his argument to show that he realised the fact that the rising of the top from that inclined position is dependent on the excess of centrifugal force at *b* over that at *d* being greater than the force of the top's gravitation to the Earth. Routh is correct as far as he goes, inasmuch as he avoids the mistake of treating either the friction or the rolling of the peg as the cause of the top rising, and ascribes this entirely to the momentum of rotation; but, as above stated, he does not seem to have realised that the manner in which the centrifugal force tends to bend the plane of rotation, *a b c d* (figure 6, p. 102), so as to bend the point *b* away from the pivot *x*, and at the same time to bend the point *d* towards *x*, is the force which lifts the top to the upright position. And he certainly had no idea of the lifting being due, not to the direct action of the motion, but to the force of gravitation which resists that motion.

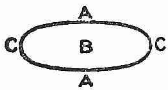


FIG. 9.—THE PEBBLE AT REST.


FIG. 10.—THE PEBBLE SPINNING.

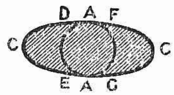


FIG. 11.

I will now pass to some phenomena which show the importance of this distinction more clearly than it is shown by the phenomena of the common spinning-top.

*The Spinning Pebble.*—Professor Perry alludes to the following phenomenon observed by Lord Kelvin and Professor Blackburn when spinning pebbles on a beach at the seaside. A C A C, in fig. 9, represents a water-worn pebble of ellipsoid shape. If given a rapid spinning motion round the axis, A A, it will change its positions until it gets on its longest axis, C C, and will then smoothly continue its horizontal spinning standing upright on that axis as in fig. 10. I have drawn fig. 11 for the purpose of pointing out that when the pebble is given the horizontal spin on the axis, A A, the particles in each end portion of the pebble—namely, the two portions F G C and D E C, in which the mean position of the particles is more remote from the centre than that of the particles within the central portion, D E G F—have a mean velocity very much greater than the mean velocity in the central portion; and the resisting force of astral gravitation, acting on each particle with a force as the square of its velocity, therefore turns the pebble about until it gets into the position in which there is least inequality in the velocity of the horizontal motion of rotation, which is the upright position.

To illustrate the above more definitely, let the pebble be so shaped that a central sphere, D E G F, may be one half of the pebble and its momentum the same whether rotating on the axis A A, or on the axis C C; and let the two remaining quarters, on opposite sides of the central sphere, be similar, and so shaped that when the pebble is rotating on the axis A A, their combined momentum may be three times the momentum of the central sphere. Then the motion of the central half, D E G F, being any quantity, say 10, that of the two quarters, D C E, and F C G, is 30; and the resistance of astral gravitation, as the squares of the velocities, is 100 in the central half and 900 in the other half of the pebble, or together 1,000. But when the pebble, if sufficiently elongated, rises and rotates on the axis C C, the momentum of the two quarters would become less than that of the central half; let us, however, suppose it to be reduced only to equality, and let us also suppose that the rotation of the pebble changes from the axis A A to the axis C C, without reduction of momentum. Then half the momentum is in the central half of the pebble and the other half in the two quarters, and the doubled velocity of rotation this requires in the central half, if the total momentum remains the same, makes the resistance of astral gravitation there 400 instead of 100, whilst in the other half it is 400 instead of 900. Thus, with the same momentum round the axis C C, as originally imparted round the axis A A, the resistance of astral gravitation is as 800 to 1,000. That is to say, astral gravitation offers only four-fifths as much resistance to any momentum round the axis C C, as it offers to the same momentum round the axis A A. And as long as the velocity which sufficed to lift the pebble to the upright position lasts, that, instead of the prostrate position, is the position of equilibrium in the combined action of gravitation.

*The Spinning Ball.*—In a paper 'On the

Irregular Flight of a Tennis-Ball'[1] (to facilitate the discussion of which I have drawn the accompanying fig. 12, p. 116) Lord Rayleigh shows that a ball, *a b*, projected horizontally in the direction of the arrow *x y*, and rotating about a vertical axis in the direction of the smaller arrows, deviates from the vertical plane *x y* towards the position *z*.

Lord Rayleigh tells us it is sometimes supposed that the above phenomenon is due to a frictional rolling of the ball on the condensed air in front of it; and he then refutes that theory by pointing out that the actual deviation is in the opposite direction to that which it requires. Lord Rayleigh then modifies a theory given by Professor Magnus, and says that the pressure in front of the ball is augmented on the side *a*, and diminished on the side *b*, by the motion of rotation, so that an excess of lateral pressure thus created on the side *a* makes the ball deviate to *z* instead of travelling to $a^1$ $b^1$. The theory is obviously plausible. If, instead of the ordinary ball, a rotating disk be allowed to swing as a pendulum, a similar deviation from the vertical plane occurs, so that its motion agrees with Lord Rayleigh's explanation regarding the tennis-ball. If, however, the rotating disk be covered with a metal case so that the air inside the case swings with the pendulum, then there is no longer the

[1] *Scientific Papers*, by John William Strutt, Baron Rayleigh, 1899, i. 344. Republished from *Messenger of Mathematics*, 1877.

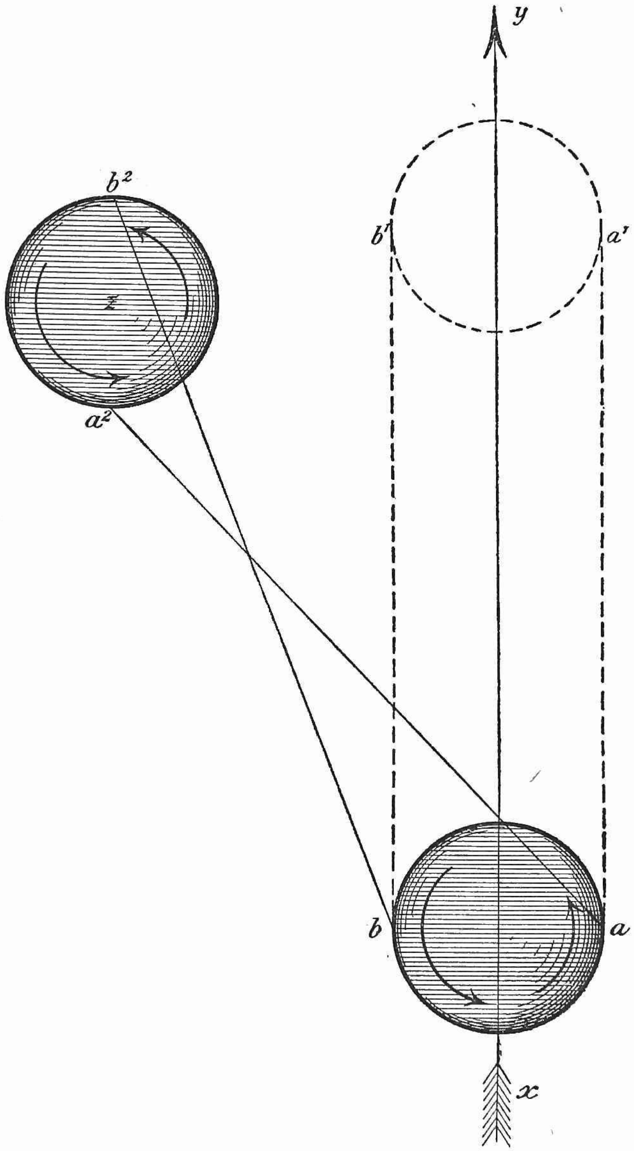


FIG. 12.—THE SPINNING BALL PROJECTED.

excess of pressure on the side *a*, to which Lord Rayleigh attributes the deviation from the vertical plane; but the pendulum will nevertheless deviate from that plane in the same manner as when the disk is exposed.

Professor Andrew Gray gave an illustration of the latter in the Royal Institution on April 29 last year (1899). That experiment not only shows that the pressure of the air is not the only force in action, but also confirms the action of astral gravitation, the resisting action of which, being as the square of the velocity of motion, resists each particle when at the point *a* more than when at the point *b*. The pressure of the resistance of the air described by Lord Rayleigh is similar to that of the resisting action of gravitation, the *friction* between the air and the surface of the ball being in fact the action of gravitation between particles in contact; but I claim that Professor Gray's experiment shows the action of gravitation to be the basis of the resisting force which causes the deviation even when that friction is eliminated as in Professor Gray's experiment.

The vis inertiæ with which the ball resists the effort made to lift it and set it in motion is the action of universal gravitation on it; and the resistance of that action of gravitation is as the square of the velocity of the motion impressed on each part of the ball.

*The Doubly Rotated Gyroscope.*—Let an ordinary gyroscope be given a strong motion of rotation, with its axis, *c d*, in fig. 13, in a vertical position and suspended from the axis of a whirling table by a cord attached to the outer ring of the gyroscope in the line of its axis, as at *b*.

If the whirling table be turned so as to en-

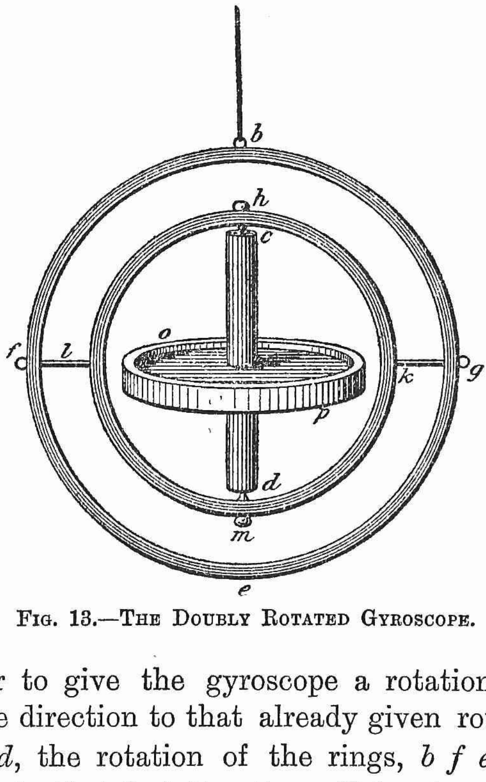


FIG. 13.—THE DOUBLY ROTATED GYROSCOPE.

deavour to give the gyroscope a rotation in the opposite direction to that already given round the axis *c d*, the rotation of the rings, *b f e g*, and *h l m k*, in that first direction will be stopped; the inner ring will turn over on the axis *f g*, together with the disk, so as to reverse the positions of the points *c* and *d* and to make the momentum of the first motion of the disk take the same direction as that given by the whirling table.

I contend that the cause of the above action is that, on the one hand, the motive force of the gravitation of the disk *o p*, tending to carry the rings *h m* and *b e* in the first direction of its motion, is opposed by the action of astral gravitation, which includes the gravitation of the rings and of the Earth; and, on the other hand, the motive force of the gravitation of the rings tending to rotate the disk in the direction of the motion given to them by the whirling table is opposed by the action of astral gravitation, which includes the gravitation of the disk and of the Earth; and I submit the following arguments in support of that view.

*First.*—In the first part of this essay I have shown that a prevalent opinion to the effect that the common spinning-top is lifted from an inclined to a vertical position by the friction of its peg against the ground is untenable.

*Secondly.*—In the experiment now under consideration the revolving force of the gravitation of the disk *o p*, of which the friction at *c* and *d* is in this experiment a part, will gradually draw the rings *h m* and *b e* round with its rotation. But, instead of leaving the gyroscope to the sole action of that rotation, given to it by spinning the disk, let momentum be given to the rings by the whirling table equivalent to that separately given to the disk and in the same direction. The disk and the rings will then smoothly rotate together. If, however, the momentum given to the rings by the whirling table be in the opposite direction to that given to the disk, then, besides the same resistance of astral gravitation as in the former case as the square of the velocity, there is an additional conflicting action between the gravitation of the disk and that of the rings, which would be as the square of double the velocity of their motions if those motions were effected, as attempted, in opposite directions; because the connecting links of gravitation between their particles would be torn asunder by a velocity double that of the mean velocity of either. The disk therefore sways from that position and turns over to that in which the maximum of momentum can be maintained with the minimum of resistance.

The action of astral gravitation just described as turning the disk of the gyroscope over is the same as that described in the first part of this essay as lifting an oblong pebble on end when given a spin as it lies on its side on the ground, where no fixed points of friction exist.

*The Raw and the Boiled Egg.*—Lord Rayleigh, in one of his lectures in March last year (1899), made two eggs spin as they lay on their sides on the table; one continued to spin in that manner, but the other quickly raised itself to an upright position, which it retained as long as a strong spinning motion lasted, because the latter was boiled and the other raw. In explanation Lord Rayleigh limited himself to the mere statement of those facts on the plea of the reason for the peculiar conduct of the boiled egg being too abstruse for a popular lecture. Astral gravitation, however, as shown in the first part of this essay in connection with the spinning of the oblong pebble (fig. 11, p. 112), gives the reason in a manner which is not, I think, too abstruse for any atten-

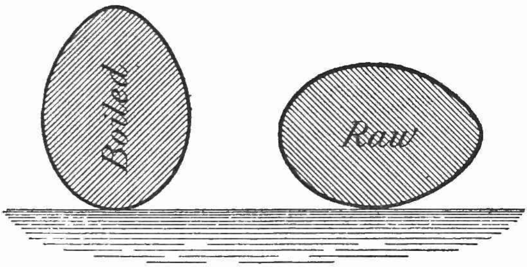


FIG. 14.—THE SPINNING EGGS.

tive reader. The different conduct of the raw egg confirms that explanation, inasmuch as, in consequence of its interior being more or less liquid, it is able to move with greater angular velocity where the resistance is least than where it is greatest; and it can therefore maintain more momentum whilst lying down than the boiled egg could do without rising on end. No perfection in the make of the gyroscope or in the oblong shape of the pebble will enable the resistance on their opposite sides respectively to be so balanced as not to turn the one over or to lift the other on end, because the Earth's rotation deranges the balance of motion, and thus creates a preponderant action at one end of the succession of instantaneous levers which enable astral gravitation to lift the boiled egg to the upright position, in which the shorter levers offer less resistance to the motion. But in the raw egg the levers bend and twist themselves round the axis of rotation instead of lifting the egg. Astral gravitation attempts the work in both cases, and under its action the egg must rise or bend the levers.

*The Rising Hoop.*—Professor Flemming, in a lecture at the Royal Institution in March 1898, sus-

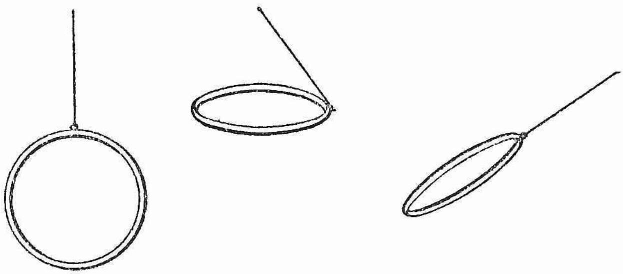

FIG. 15. FIG. 16. FIG. 17.

pended a hoop by a string and showed that the rotation given by a rapid rotation of the string caused the hoop to change from its vertical position (shown in fig. 15) to the horizontal position (shown in fig. 16). And in explanation of that movement he expressed the opinion, which has, I think, been generally prevalent, to the effect that the change of position is effected because the vertical position is an unstable, and the horizontal position the only stable position of rotation, though no reason for the latter position being one of stability appears to me to have been given. It seems to have been assumed to be a position of stability merely because the hoop places itself in it. The fact, however, is that not only the theory now under consideration,

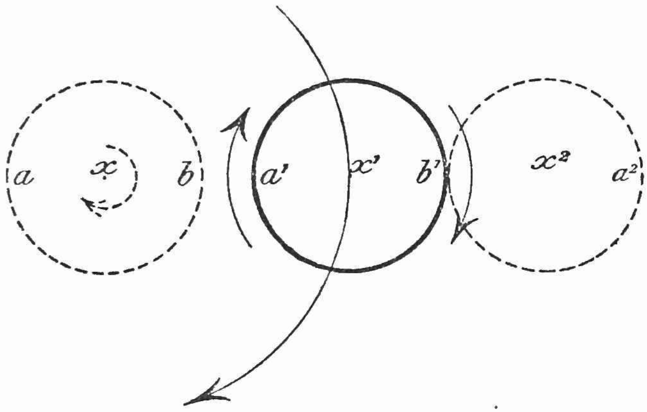


FIG. 18.

but also experiments I have made for the purpose of testing it, show that the horizontal position (fig. 16) is a transition stage from the vertical position of fig. 15 to that shown in fig. 17, in which the hoop revolves like a stone at the end of a string.

Against the supposed stability of the position shown in fig. 16, I say that the Earth's motion tends to destroy the stability of that horizontal

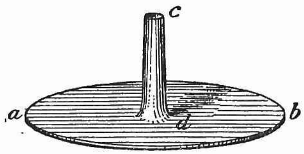


Fig. 19.

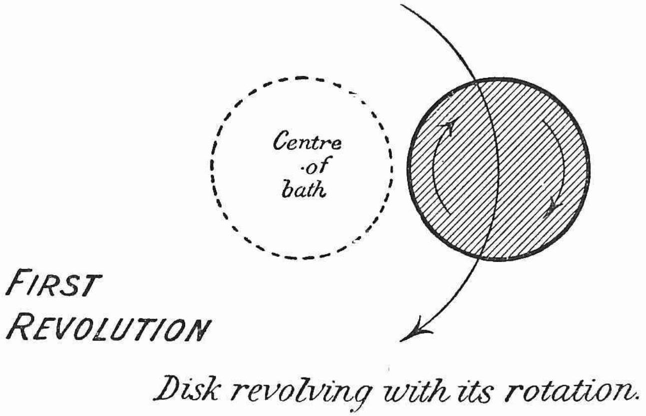


Fig. 20.

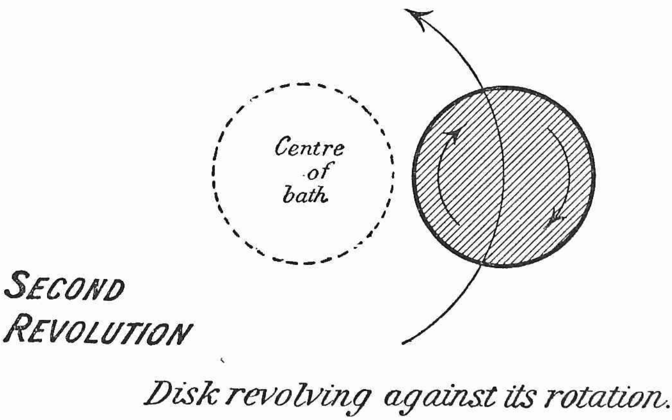


Fig. 21.

In the transition the disk falls towards the centre of the bath, and, passing behind instead of in front of it, comes away again with its revolution reversed.

position in the same manner as it destroys that of the vertical position.

Let the dotted circle, *a b*, in fig. 18, represent the hoop in the horizontal position rotating in the direction of the dotted arrow with its centre $x$ directly below the point of suspension, and the side *a* moving in the direction of the Earth's rotation. The greater resistance at *a* then tends to make the hoop sway towards the position $a^1$ $b^1$, as shown in connection with fig. 6, page 102. It seems to me quite possible in theory that a stable motion of rotation round the axis $x^1$ in combination with a revolving motion round $x$ might be created, though the Earth's motion is constantly changing the plane of the hoop's motion, and consequently tending to derange its stability. But, as in the hoop's own motions of rotation and revolution the resistance at $b^1$ is greater than at $a^1$ in proportion to the squares of the velocities, it constantly tends to bring the hoop back to the position $x$, from which the Earth's motion has made it sway. Each time the point at which the suspending cord is fixed passes $b^1$ there is a chance for the changing plane of the hoop to make the resistance of $b^1$ slightly upwards and that at $a^1$ slightly downwards, and thus to turn the hoop over with the point of suspension as a pivot so as to bring $a^1$ to the outside position, as at $a^2$; thus making the point by which the hoop is suspended the inside instead of the outside point in the motion of revolution.

The range of my experiments has been limited; but, for the purpose of testing the explanation given in connection with the foregoing (fig. 18), I took a metal disk, *a b* (fig. 19), suspended by a cord attached to a short bar, *c d*, projecting at right angles from one side of its centre, and allowed it to hang in a round bath containing water, and to prevent the disk from being lifted straight upwards clear of the water by the shortening of the cord as it twists I substituted a thin metal rod for enough of the central part of the cord to prevent the twisting of the remaining portions of the cord shortening enough to lift the disk beyond the surface of the water.

Rapid motion suddenly given will fling the disk out of the water, but by commencing with a slow motion and gradually increasing the velocity the disk soon acquires a revolving motion in the same direction as that of its rotation, just as described in connection with the hoop. But the resistance of astral gravitation, increasing as the square of velocity and acting on the combined velocities of rotation and revolution on the outside half of the disk, twists that outside half backwards in a spiral round the original axis, thus reversing the direction of the motion of revolution; and conditions of stability appear to be attained by those combined motions of rotation and revolution, as velocities of motion greater than requisite to fling the disk out of the bath under the first direction of revolution merely change the velocities of motion without showing any tendency to affect their stability. The water in the bath revolves with the *rotation* of the disk, whilst the disk revolves against the course of both, cutting through the water, which, revolving in the opposite direction, rushes against it with the combined force of the two opposite motions, though the disk itself keeps the water in motion. Figs. 20 and 21 show the successive stages of the disk's motion. The dotted circle shows its first position rotating without revolving. With the motions shown in fig. 20 it is evident that there cannot be equal velocity of the combined motions at all points of the disk; whereas the motions shown in fig. 21, which are arrived at by the transition above described, allow such equality of motion to be arrived at.

*The Wabbling of the Hoop.*—Sir Robert Ball, in a lecture at the Royal Institution in January 1899, showed the hoop experiment above commented on in connection with Professor Flemming's lecture; and he specially drew attention to a peculiar wabbling motion as the hoop rises from the vertical to the horizontal position shown respectively in the foregoing figs. 15 and 16. Sir Robert Ball gave no explanation of the wabbling, and I had not before given it any consideration but now offer some reasons for considering that it may be due to the action of the earth's rotation.

Figure 22 shows the starting position. The resistance at S, being relatively increased by its motion being *with* whilst that of the opposite point N is *against* the Earth's rotation, draws S towards the axis W *k*, and drives N from it; so that by the shortening of the radius *c* S, and lengthening of the radius *c* N, the forces acting eastwards and westwards respectively may be equalised by making the eastward motion in the smaller segment *p* S $g^1$, in fig. 23, counterbalance the westward motion in the greater segment P N $g^1$.

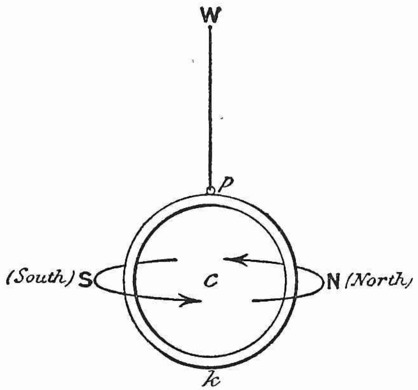

FIG. 22.

But the resistance to the revolution of *k*, in fig. 23, with a greater velocity of motion than the opposite point, *p*, holds it back and throws *p* forwards, placing the axis of rotation south of both those points as at *h g* in fig. 24.

As S comes round to the east, as shown in fig. 25, and N goes to the west, the effect of the Earth's rotation, which changed the relative velocities of motion at those points when they were north and south, ceases, and $p$ and $k$ fall towards their normal positions, $p^1$ and $k^1$ respectively, until the approach of N to the south and S to the north renews the former action, and thus creates a wabble in each rotation until the hoop reaches the positions shown in fig. 26.

The result of experiments I have made is to show that the horizontal position which the hoop appears to get is an optical illusion. The fact is that all positions of the hoop in a revolution of the point $p$ in fig. 26 are visible at the same time, and their combination gives a horizontal line. The upper edge of the apparently horizontal hoop is only the point of the hoop to which the string is attached, and the lower edge is only the opposite point of the hoop. I have clearly shown that to be the case by colouring the half of the hoop next the string blue and the other half red; and then, when the position which appears perfectly horizontal is reached, on looking down vertically on the hoop, a complete circle of blue is seen floating over a red circle which shows itself along the edges of the blue, which latter obliterates the red in the same line of sight by passing round too rapidly for the sight to reach the red which occupied the same line of sight the previous instant. When looked at horizontally, two distinct hoops are seen, the upper one blue, and the

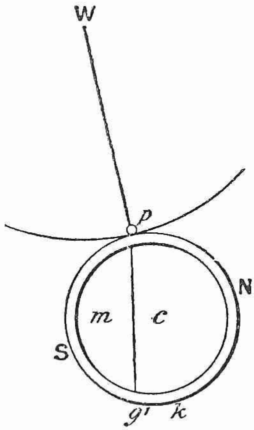


Fig. 23.

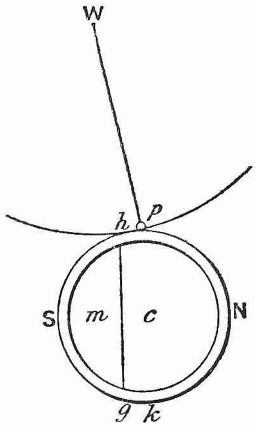


Fig 24.

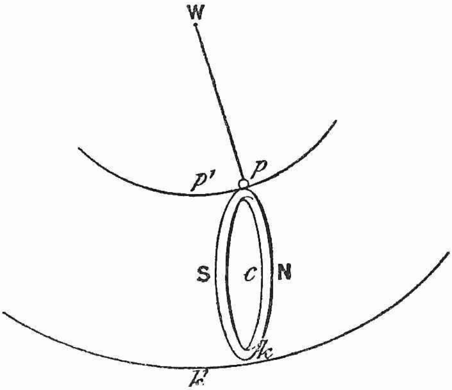


Fig. 25.

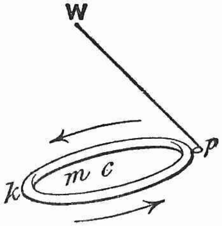


Fig. 26.

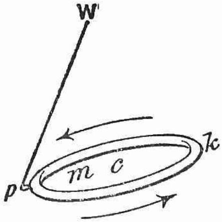


Fig. 27.

The Hoop suspended by a Rotating Cord. (Its first position is shown in fig. 22, p. 128.)

under one red, connected only by vague colouring about their centres.

I coloured the hoop as above stated in the expectation that, on reaching the horizontal position, the colours would blend, except that a spot of blue would be left on one side and a spot of red on the other side of the hoop, marking a tidal action, resulting from the Earth's rotation; but, as above shown, the hoop, if evenly balanced, cannot reach the horizontal position in that experiment; the reason apparently being that, as the Earth's gravitation is a component of astral gravitation in its action on the hoop, there must always be a residue of force acting towards the Earth, no matter how much the force of astral gravitation is increased by increasing the velocity of the hoop's rotation.

For some time the blue and the red hoops above described retain their relative positions, the former rotating and revolving exactly above the latter. But at length they overlap each other, more or less vertically as well as horizontally. That separation of the colours on opposite sides of the hoop is quite clear, and in the experiments I have made when the rotation is with that of the Earth the red seems to spring clear of the blue in the first instance from south through east to north, leaving the blue at the same time clear of the red on the west side of the hoop; and when rotating against the Earth's rotation the red seems to first spring from north, through east, to south. The red excrescence on one side and the blue on the opposite side of the hoop give the appearance of tidal action of the character I had been looking for, but east and west instead of north and south. It is not easy to keep it in sight, because, when it appears, the motion of the hoop is approaching a critical stage in a gradual transition from rotation to revolution, and the phenomenon disappears by the red taking an outward and the blue an inner circle in the motion of revolution shown in fig. 18, p. 123. It seems to me to be only at the moment when, as shown in fig. 18, the hoop springs with a jerk from under the cord that it reaches the position shown in fig. 27. The facts seem to show that, no matter how accurately balanced the hoop may be in the first instance, the leverage of astral gravitation must change it from its vertical to its horizontal position (shown in figs. 15 and 16 respectively). And that same action of astral gravitation changes the hoop's motion of rotation (shown in fig. 16) to the motion of revolution (shown in fig. 17); and in the latter transition creates a tidal action, as in the transition from the vertical to the horizontal position.

The conflicting action of the vis viva created by the direct action of the motive force and the gravitation which resists the motive force puts a stress on a rigid axis, or twists a pliant axis, just as the conflicting action between a carriage and a pair of horses puts tension on traces which connect them. The twisting of the cord is as obviously the immediate cause of the rotation of the hoop as is the tension of the traces the immediate cause of the motion of the carriage. But a discussion as to the character of the horses or the obstructions which hinder the motion of the carriage is not affected by a proof that the tension of the traces corresponds with the motion of the carriage and is the immediate cause of that motion; nor does a proof that the motion of the hoop corresponds with the twisting of the cord affect the question as to the nature of the conflicting forces which cause the cord to twist.

*Hall Tennis.*—Professor Boys, in a lecture at the Royal Institution in January this year, gave illustrations of the curve line in which the ball moves in the game of hall tennis. But, as the side moving with greatest velocity in his illustration is obviously forced to do so by the direct action of the blow struck on that side, the resisting action of gravitation can only be indirectly illustrated in such phenomena. In the spinning-top the same velocity is equably given all round the axis in the plane of the spinning motion; and it directly illustrates the manner in which the action resists the force with which the direct action of the Earth's gravitation endeavours to make the top fall.

*The Rotating Band.*—A phenomenon shown by Professor Boys, which does serve as an illustration of the direct action of the resisting force of gravitation, was an india-rubber band hanging on a roller as shown in fig. 28; on the roller being made to rotate rapidly the band stretches, as shown in fig. 29, as if a weight shaped like the roller had been placed in it. The resisting action of gravitation acting as the square of the velocity of each atom of the band pulls back against their motion and stretches the band exactly as a faster velocity of the rotation of the planet Saturn would result in a stretching of its rings to larger circumferences.[1] The lopsided stretching of the band is a tidal action due to the close proximity and paramount action of the Earth's gravitation; but it is similar to the tidal action of the sun and moon on the ocean as it is revolved with the Earth's rotation.


Hanging at Rest. FIG. 28.


Revolving. FIG. 29.

THE ELASTIC BAND.

*The Falling Slip of Paper.* Another phenomenon shown, but left unexplained, by Professor Boys, was the motion of some pieces of paper allowed to fall from the ceiling of the lecture-room. Professor Boys said it seemed natural

[1] The theory of that action is explained in Proposition XXV., book x., of *The Ocean*.

enough that the resistance of the air should prevent a sheet of paper from keeping a flat position (as in fig. 30, in which the line A B is one edge of the paper); but that the fact that when it has turned to a sloping position (as in fig. 31) it does not continue to fall in that slope is not explained. The fact, however, is that to change from the level to the sloping position the point B must have moved with greater velocity than the point A; and the resistance, being as the square of the velocity, stops the relatively greater motion of B with a jerk, tending to draw it back and give A a relative increase of down-


FIG. 30.


FIG. 31.—THE FALLING SHEET OF PAPER.

ward motion. Precisely that same action can be seen in a spinning-top, and still more definitely illustrated in a gyroscope. When the excess of resistance acts at B, the line A B is a lever for its action with A, for a fulcrum to which an extra downward force is given by the upward pull at B. In the spinning-top the point of the peg is that fulcrum and gives a firm leverage, which enables the upward pull at B$^{1}$ to lift the top to an upright position, as already explained in this essay. More than twenty years ago I had a double gyroscope made for the express purpose of getting a leverage of resistance to downward motion at the points A and B simultaneously, expecting that if placed in a pair of scales part of the weights required to support it when the disks were not spinning might be removed when the two disks were spinning in opposition, without the scale being pressed down, as it would be if the disks were not spinning, the upward pull at each of the points A and B equably exceeding the downward pull of gravity, and therefore each reciprocally acting as a fulcrum to the leverage of the other. But whenever, in the experiments I made, the opposition was equably arranged, the vibration became too much for the pivots of the gimbals, and the disks fluttered out of them before the weighing could be fairly tested.

*The Hydraulic Ram.*—Another point in the above lecture invites comments which may, I think, not inappropriately be included in this essay.

Professor Boys, when explaining the action of the hydraulic ram, flouted the new theory of vis inertiæ and paraded his adherence to the old by declaring the momentum of the water which forces it upwards to be the inertia of its motion. Neither Professor Boys nor any other acknowledged teacher of natural philosophy disputes the fact that the action is due to the momentum of the water, which is as the velocity, and that the vis viva, or force of the motion, is as the square of the velocity; so that the action of the ram may be properly described as due to the vis viva of the motion of the water as it flows through the pipes. I beg the reader to consider well as to whether there must not be erroneous theory in one of those orthodox views, seeing that Professor Boys is able correctly, on the supposition that the so-called First Law of Motion is a true law of nature, to declare that the acknowledged vis viva of motion is the inertia of the moving body, using the term inertia to represent what Newton termed vis inertiæ. The fact that the views held by teachers of natural philosophy allow the same direct action of motive force to be termed either vis viva or vis inertiæ, just as a momentary fancy may incline the speaker, seems of itself to show the existence of some theoretical error. Some writers now distinguish between *inertia* and *vis inertiæ* by treating the former as the property by virtue of which matter resists, and the latter the resistance itself. I do not think Newton ever made or suggested such a distinction, but invariably used the term vis inertiæ. Supposing, however, the asserted First Law of Motion to be a true law of nature, the distinction is really an axiomatic statement, expressing the mere matter of fact that the inertia of the motion of the water in the pipe of the hydraulic ram exerts a force of inertia, or vis inertiæ, which is as the square of the velocity of the motion. That makes the vis inertiæ of the motion of the water on which the action of the ram is dependent identical with

Newton's vis viva of the motion, and constitutes a *reductio ad absurdum* of the First Law of Motion.

Vis viva and vis inertiæ are *opposite* aspects of the same stress, and never the *same* aspect of any stress, as they are made to be by Professor Boys's perfectly 'orthodox' explanation of the hydraulic ram.

*The Revolving Force of Gravitation.*—Professor Turner recently commenced an interesting lecture at the Royal Institution by making an uncompromising profession of faith in the principles under which he was drilled to pass examination for honours in science, and by implication scouted the revolving action of gravitation as unworthy to be noticed by any one educated in what a few generations of scientific men have believed to be the truth.

The pose then assumed by Professor Turner in presence of the new philosophy made it all the more refreshing to hear Professor Poynting describe, in a subsequent lecture, an experiment he had made for the purpose of testing that revolving action; thus advancing at least so far as to admit that the subject is not undeserving of consideration. But, notwithstanding the delicacy of his experiment, no satisfactory result could be expected from the leverage exerted by so small a sphere as that used by him, which did not appear to be of more than two inches in diameter at the most.

In order to exert a direct force of gravitation, even so great as to be only one billion billion times less than that of the Earth, the small sphere, if of the same specific gravity as the Earth, would have to be 5 inches in diameter, and would weigh $13\frac{1}{2}$ lbs.

The masses being as the cubes of the diameters, and the revolving forces being also, at equal velocities of motion, as the cubes of the diameters (the vis viva of motion being as the square of the velocity of motion); therefore, at equal velocities the revolving forces of the small sphere and the Earth would have the same ratio to each other as their direct forces.

As in practical experiments a particle close to the surface of the small sphere is, roughly speaking, 100,000,000 times farther from the centre of the Earth than from that of the small sphere, the Earth's greater relative force is (in the position of our experiments) reduced to the square root of its above-mentioned excess of one billion billion times the force of the small sphere, thus making it, in experiments close to the surface of the small sphere, only 100,000,000 times greater than the force of the smaller sphere.

That is the ratio of their respective diameters, which is shown to be the ratio of their respective forces of gravity on their respective surfaces by Newton's Prop. LXXII., book i., of 'The Principia.'

The small sphere would therefore, as above shown, exert in the experimental arrangement a direct force 100,000,000 times less than that of the Earth; and if rotating with the same velocity of motion as the Earth, its revolving force would also, as above explained, be 100,000,000 times less than that of the Earth.

To get a revolving force on the surface of the small sphere equal to that of the Earth, a velocity 10,000 (the square root of 100,000,000) times faster than that of the Earth's rotation would be required; or, to equal the direct force of the Earth holding any object to its surface, a velocity 170,000 times greater than the Earth's (as the Earth's direct force is 289 times greater than its revolving force at the equator). That would require the equatorial velocity of the small sphere to be more than 7,000,000 miles an hour.

Such a velocity would dissipate the atoms of, or melt, any known substance. Effective action of the revolving force of the small sphere could, however, be immensely facilitated by bringing it to bear on the rim of a fly-wheel balanced on a pivot. But even if a leverage as 170,000 to 1 were gained in that manner, a velocity of 1,460 feet a second would be wanted to overcome the resistance of the Earth's gravity represented by the pressure of the fly-wheel on its pivot.

In space, where bodies are equally balanced between opposing forces of gravitation, the slightest action of a revolving force produces its effect; but on the surface of the Earth the paramount action of its force has to be counteracted before any effective action can be apparent. Newton's argument applied to the direct force is equally applicable to the revolving force of gravitation. He says (Prop. VII., book iii.): 'If it is objected that, 'according to the law, all bodies with us must 'mutually gravitate one towards another, whereas 'no such gravitation anywhere appears, I answer, 'that since the gravitation towards these bodies is 'to the gravitation towards the whole Earth as 'these bodies are to the whole Earth, the gravita- 'tion towards them must be far less than to fall 'under the observation of our senses.'

That argument is, as I have shown above, equally applicable to the revolving force of gravitation. The fact that no sensible effect could, in Newton's time, be shown to be produced by the gravitation of the small sphere was then quite as strong an argument against the general law of the direct action of gravitation as the same argument now is against the subordinate law of its revolving action. The question is as to whether in any practicable experiment the size of the above-mentioned small sphere can be sufficiently increased to give the increased leverage necessary for the production of a sensible effect without a velocity of rotation too great for the material of the sphere to bear.

*The Resisting Action of Gravitation.*—Let E and A, in fig. 32, represent respectively the positions of two bodies in space. Or let them represent the respective centres of gravity of any two combinations of bodies. Or let the Earth be at E; and let B be a body just so far removed that it may be in equilibrium between surrounding forces of gravitation.

The excess of the Earth's force of gravitation acting on B at the point B¹ exceeds that at the point *d* inversely as the cube of the distance E B. And the opposing stress of gravitation which keeps B in equilibrium, represented by the point A, is in the same ratio greater at *d* than at B¹. But whatever the absolute force of the excess dependent on the distance E B may be at the points B¹ and *d*, it must vary as the ratio which the diameter B¹ *d*

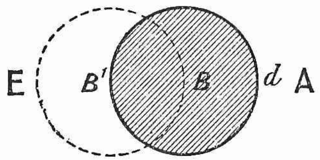


FIG. 32.

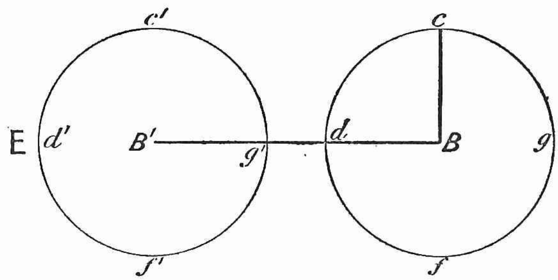


FIG. 33.

bears to the distance E B varies. We may therefore disregard the distance from which the force E acts, and deal solely with the relative amounts of the forces E and A at any point whose position is measured from B.

Now, instead of equilibrium, suppose the force

E to be sufficiently in excess to draw B in any fraction of time from B to $B^1$, just one half diameter nearer to E. Then that distance, of one half diameter, is not only the measure of the difference between the force E and the force A at the point B, but also of a further increase of the force E at the point $B^1$, and at the same time a corresponding decrease of the force A at that point.

Therefore, if the distance B $B^1$ be 16 feet and the time taken to traverse that distance be one second, the distance traversed in the second second will be increased a further amount of about 16·1 feet by the increase of the force E, and another amount of about 15·9 feet (together exactly 32 feet) by the reduction of the opposing force A, making the fall in the second second 48 feet; or in the two seconds 64 feet; thus making the distance traversed in the fall towards E as the square of the time.

The distance traversed in the first second (given above as 16 feet for the sake of illustration, the actual fall near the Earth's surface being 16·1 feet) depends on the mass of the body E and the inverse square of its distance; but, however small or great it may be, the law for its increase in the second second is the same.

Though the Earth has a predominant action in the foregoing respect in its own sphere of influence, in which the above example is given, every body in the universe is exerting exactly the same action; and that action of the Earth is the Earth's contribution towards the combined action of gravitation, which resists the direct action of every motive force with an opposing force which is as the square of the velocity of the motion effected.

The Earth's gravitation is an intrinsic part of the resisting force wherever motion is impressed on any body not by gravitation but by a purely external force. Whether the body so acted on be lying on the surface of the Earth or be placed in any distant part of the universe, the Earth's action is intrinsically the same, differing only in degree. If a ball be thrown by the hand the Earth's resisting action is precisely the same in whatever direction the ball be thrown. That it resists any motion imparted by the hand downwards, as well as upwards, or horizontally, is demonstrated by the following argument.

Let the Earth be at E (fig. 33, p. 142); the ball to be thrown by the hand from B; and *c* the position to which it might be thrown in one second if unaffected by the Earth's gravitation.

Now let $B^1$ be the position to which the Earth's gravitation would make the ball fall in one second if left free at the point B instead of being thrown in the direction *c*; then $c^1$ is the position at which the ball will be found at the end of the second. The same argument applies if the ball be thrown in any of the directions *d*, *f*, *g*. It will at the end of the second be found at $d^1$, $f^1$, or $g^1$, respectively. The effort made by the hand to throw the ball effects the same quantity of motion in whatever direction the ball is thrown; and that motion effected by the direct action of the impressed force is the motion which is resisted by the combined action of gravitation with a force which is as the square of the velocity of the motion effected. The force exerted by the hand, which may cause the ball to be at $d^1$ instead of at $B^1$ (in which latter position gravitation endeavoured to place it) is resisted by gravitation exactly in the same manner as that which may cause it to be at $c^1$, or at $g^1$, instead of at $B^1$.

The foregoing gives a basis for showing points of agreement and points of difference between my views and those of Newton's First Law of Motion and definition of vis inertiæ.

1st. As regards the body falling from the point B to $B^1$ (in fig. 32, page 142) there is no difference of opinion. I explain the phenomena from a different point of view; but there is no difference as to the resulting effects.

2ndly. As regards the ball thrown from B (fig. 33) towards $c$, $d$, $f$, or $g$, being found at the end of the second at $c^1$, $d^1$, $f^1$, or $g^1$ respectively; my explanation is intrinsically the same as that given by Newton's parallelogram of forces; but I say that the force of gravitation which tends to draw the body from B to $B^1$ is, by that action, endeavouring to restore a position of equilibrium which has been disturbed by the action of some external force; and that identical force of gravitation resists the force of projection B C, because the latter is also causing a disturbance of the equilibrium of gravitation. That opposing action of gravitation therefore makes the force of projection give a gradually decreasing velocity; and it is always the equal and opposite stress to the vis viva of motion until the force of projection is expended.

Gravitation is throughout the duration of the motion giving a velocity increasing as the square of the time for the restoration of equilibrium, and is also resisting the impressed motion of projection as long as it lasts, with a force as the square of the velocity of that impressed motion.

A logical application of Newton's parallelogram of forces, in the manner in which I have above stated it, seems to make it evident that the impressed force is equally resisted by gravitation in whatever direction the motion may be impressed. And as that action of gravitation is reciprocal between the body on which the force impresses motion and all other bodies, it seems to be mere matter of fact that the body does not by any innate property of its own tend to perpetuate the motion impressed by the extraneous force, but acts in combination with all other bodies in resisting the motion.

Let a cannon-ball be projected upwards with an initial velocity of 2,898 feet per second, and grant, in accordance with accepted theory, that gravity reduces that upward velocity reckoned from the point of projection 32·2 feet per second. The motion will then be expended at the end of 90 seconds, after carrying the ball 130,410 feet upwards, a height of about twenty-four and a half miles. But in those 90 seconds gravity has caused a downward motion of 130,410 feet (16·1 feet multiplied by the square of 90), so that if the ball is at a height of twenty-four and a half miles at the end of the 90 seconds the motion due to the force of projection is 49 miles with an initial velocity of 2,898 feet. That, I say, must be the quantity of motion effected by that force of projection, in whatever direction the ball be projected, if accepted theory is right in supposing that an initial velocity of 2,898 feet per second would (in the absence of atmospheric friction) be sufficient to project a ball to a height of twenty-four and a half miles.

To lift the ball at all, the force must be greater than gravity, for a force equal to gravity is required merely to keep the ball from falling. Gravity causes a fall of 16·1 feet in a second; therefore, to raise the body 16·1 feet the force must do twice the work of gravity, or 32·2 feet of work against 16·1 feet done by gravity; and to rise the 16·1 feet in the second the body must have an initial velocity of projection at the rate of 32·2 feet per second, gradually slowing down to nothing at the end of the second. That holds good for all distances. The velocity of 2,898 feet per second above dealt with is 90 times the velocity given by gravity in the same time, and will therefore do 90 seconds of work against gravity before being exhausted. In those 90 seconds the force of projection does 49 miles of work, as above shown, whilst gravity does $24\frac{1}{2}$ miles, whether the forces act with, or against, or across each other. The work done by gravity in the 90 seconds is the same whatever may be the direction of projection; and, if so, then the work done by the force of projection is also the same whatever may be the direction of projection. If the force of projection does 49 miles of work in the vertical direction, that is also the amount of work which it can do in any other direction; and having done that work its action ceases. Any reduction of the 49 miles due to friction of the air must be greater in the horizontal than in the upward direction, reducing the 49 miles in the horizontal direction more than the 49 miles in vertical direction; but the resistance of gravity to the force of projection is shown by the argument in connection with fig. 33 to be the same in all directions.

Instead of projecting the ball from a position on the Earth's surface, let us suppose it removed so far off as to be in equilibrium between surrounding forces of gravitation. The forces of gravitation holding the ball in that position of equilibrium must resist the force of projection, taking it from that position just in the same manner as the forces of gravitation which held the ball in its first position on the surface of the Earth, resisted the force of projection acting from that position. In both positions the ball was held by universal gravitation, not only by that of the Earth. It is the combined action of gravitation which resists the motion; and in that combined action the Earth's gravitation resists the motion, whatever may be its direction and whatever may be the position from which it may be projected.

I say that motive forces are to each other as the squares of the velocities they cause. Newton's Second Law of Motion says in effect that motive forces are as the quantities of motion they cause. We have before us two velocities, 2,898 feet and 32·2 feet, a ratio of 90 to 1; making me require the forces to be as 8,100 to 1; but the feet of work done by the forces are, as above shown, 260,820 and 32·2 respectively, which is also as 8,100 to 1, placing my argument in perfect accordance with that doctrine of Newton's Second Law of Motion.

Newton's Third Law of Motion, to the effect that every action of force is opposed by an equal reaction, forms, in conjunction with his Law of Gravitation, the basis of the argument by which I have shown vis inertiæ and gravitation to be opposite aspects of the same stress, and interchangeable terms for the respective aspects; and also of that by which I have shown that the tidal action of the moon's gravitation on the Earth is a proof of a revolving action of the Earth's gravitation in the orbit of the moon. The same grasp of gravitation which holds a table on the floor also carries it round the Earth's axis with the Earth's rotation, precisely as a similar grasp of gravitation holds and revolves the moon.

## *SIXTH ESSAY*

### SIR ISAAC NEWTON AND MODERN CHEMISTRY

Having in the course of a recent conversation expressed an opinion to the effect that some of the experimental lectures at the Royal Institution and at the Society of Arts during the past session (1896-97) tended in the direction of confirming the gravitation theory of chemical affinity, and that little more than thirty years ago no such theory existed, I was surprised by being told in reply that, though renewed attention has recently been directed to that theory, it was by no means new, but had been suggested by Sir Isaac Newton in his treatise on Optics. I did not, in the first instance, doubt the accuracy of the statement, but when thinking of it afterwards it seemed so much at variance with my impression of the position of the question that I have referred to the above treatise and find not only explicit confirmation of my impression of Newton's view, but also a more important passage expressing an opinion which bears indirectly on the question, though Newton's corpuscular theory of light made it impossible for him to see the goal to which his argument would tend on the abandonment of his idea of light being a material substance. I will, however, before attempting to give the said passage the importance which it seems to me to merit in modern science, first deal with those of Newton's arguments which bear directly on the question of chemical affinity, and which, in fact, clearly refute the idea of his having supposed it to be due to the action of gravitation.

Newton says: 'There are agents in nature able to make the particles of bodies stick together by very strong attractions. And it is the business of experimental philosophy to find them out.' [1]

He also says that nature performs 'all the great motions of the heavenly bodies by the attraction of gravity, which intercedes those bodies, and almost all the small ones of their particles by some other attractive and repelling powers, which intercede the particles.' [2]

One more quotation will suffice for this point. He says: 'When *aqua fortis* dissolves silver and not gold, and *aqua regia* dissolves gold and not silver, may it not be said that *aqua fortis* is subtile enough to penetrate gold as well as silver, but wants the attractive force to give it entrance?' [3]

There may be passages in Newton's works which, if standing alone, might possibly be con-

[1] Horsley's edition of Newton's *Works*, iv. 255.
[2] *Ibid.* [3] *Ibid.* 247.

strued as supporting my informant's statement. But the foregoing extracts amount to an explicit statement of opinion to the contrary, and show that Newton did not suppose the forces of cohesion and chemical affinity to be identical with gravitation. If he had supposed cohesion to be the simple action of gravitation, he would not, as in the first of the above extracts, allude to it as a force which remained to be found out. The second extract forms an express statement of opinion to the effect that the atomic motion by which substances intermix under the action of chemical affinity is not caused by gravitation, but by some other forces. And the third extract is to the same effect; for, though his laws of gravitation require that each material atom must exert the force of gravitation in the same manner, he nevertheless considers that they differ as regards the force which causes the action of chemical affinity. The idea that Newton supposed gravitation to be the cause of chemical affinity is therefore erroneous. The views he expresses in the above and other passages may rather be considered to form the basis of the electro-chemical theory which supposes the force to be exerted only between atoms endowed on one part with positive and on the other with negative electricity. I do not understand that this electro-chemical theory was at any time considered to be among Dalton's discoveries, or that it was more advocated by him than by other philosophers of his time. What are specially known as Dalton's discoveries, whether made entirely by him or partly by others, do not affect the question now under consideration, for they fit into the gravitation at least as well as into the electro-chemical theory. The gravitation theory does not require the supposed difference of positive and negative electricity, but merely that the atoms in any two substances must be so shaped that they cannot in either one of those simple substances approach each other so closely as those of a mixture containing a given relative number of atoms of each of the substances; for in such case a compound substance will be formed whenever the simple action of gravitation between the different atoms has a chance of effective action. The force is the force of gravitation acting between the individual atoms, and has the opportunity of effective action, spreading throughout the entire masses of the two substances directly the masses touch at any point. There cannot be a commencement of effective action without contact at some point, because, until in contact, the action of gravitation between the atoms is not dependent on their shapes, and there is therefore no reason why atoms of one shape should approach each other more than atoms of another shape; but when once they are in contact, then the amount of the reciprocal action of gravitation which can become effective between them depends on their shapes, and may be infinitely greater in one case than in another.

If Dalton's discoveries had been to the effect that any substance chemically compounded of two other substances was formed by a union of an equal number of atoms of each of the two substances, that would, I should say, support the electro-chemical rather than the gravitation theory. But the facts are not so ; on the contrary, different substances are found to combine in all sorts of different proportions. There are perhaps almost as many different ratios of chemical combination as there are substances which will combine by the action of chemical affinity. And this fact, though not a proof against the electro-chemical theory, appears more strongly to support the gravitation theory, because the relative number of atoms of the substances which will fit together to form the compound substance must, under that theory, vary according as the shapes of the atoms vary. Then, also, the fact that some salts when mixed with water do not increase the bulk of the water more than to the extent of the quantity of water which already formed a constituent part of the salt, tends rather in favour of the gravitation than of the electro-chemical theory, because the latter gives no *à priori* reason why any two substances combined should occupy less space than the two substances require separately ; whereas the former theory makes it natural that the relative shapes of the atoms should sometimes result in a combination of the two being packed into less space than required by the two substances when uncombined. It does not by any means follow from this that chemical combination should always result in reduction of bulk under the action of the gravitation theory, because compound atoms or molecules formed by that action might leave even larger spaces between the molecules in the compound than between the atoms in either of the separate substances. The compound substances may be formed either by a simple interspersion of the atoms of the two substances without actual combination, or by the formation of new molecules or larger atoms in the compound, which might possibly be so shaped as to increase rather than diminish the bulk.

It is to the latter of those two classes of compounds only that the term 'chemical combination' is technically applied, the former being distinguished from it by the term 'mechanical mixture.' I do not see that it can reasonably be denied that the latter term is properly applied to that interspersion of atoms of different substances; but it is too arbitrary an interference with the ordinary use of language to attempt to limit the term to such a mixture as above described. A salad is a mechanical mixture, though its ingredients will not, like the ingredients of the atmosphere, intersperse themselves as requisite to form the salad without artificial assistance; and, on the other hand, the 'chemical force' which causes 'chemical combination' is merely the same force of gravitation which causes what is technically termed 'mechanical mixture.'

The gravitation theory gives an *à priori* reason for some compound substances being able to combine chemically or intersperse with substances which could not combine with either of the separate substances which form the compound, as the shape of the compound molecule may allow of closer contact with it than with either of its constituent atoms.

I now pass to the subject which forms, as already stated, the chief point on which I purposed to offer some comments.

Newton says: 'The changing of bodies into light, and light into bodies, is very conformable to the course of nature, which seems delighted with transmutations. . . .

'Why may not nature change bodies into light, and light into bodies?'[1]

And at another time he says: 'It can no longer be disputed whether colours be the qualities of the objects we see; no, nor perhaps whether light be a body.'[2]

To my mind it is clear that the reason why

[1] Horsley's edition of Newton's *Works*, iv. 241, 242.
[2] *Ibid.* 305.

Newton adhered to the corpuscular theory of light, insisting on light being a material substance, was that, on the one hand, he found himself forced to recognise the transmutation of material substances into light, and, on the other hand, it does not seem ever for a moment to have entered into his mind to suppose the possibility of the existence of transmutations between matter and force. The transmutations, in which he declared nature to delight, were limited to transmutations from one form of force to other forms of force, and from one form of matter to other forms of matter ; but he barred himself from allowing nature to delight in the wider transmutations between forms of matter and forms of force. If he had for a moment allowed himself to entertain the idea of the powers of nature being extended to the wider transmutations, it seems to me evident that his clear recognition of the fact of matter being transmuted into light would not have prevented him from adopting the theory then advocated by Hooke, and now universally accepted, to the effect that light is vibratory motion and not material substance. But it would not have been possible for him to accept that theory without at the same time asserting that every ray of light that surrounds us proclaims nature's delight in transmutations between matter and force. And, instead of claiming only that *in the beginning* God created atoms of wondrous hardness and durability,[1] he would have proclaimed that God is to-day as active a creator, as well as controller and dissolver, of matter as ever He was millions of years ago.

Among the important advances in scientific knowledge to which evidence has been pointing during the course of the splendid experimental lectures to which I have above alluded, there has been a tendency in the direction of showing that material substance exists only within a limited range of temperature; that is to say, that a sufficient rise or fall of temperature will transmute any form of matter into immaterial force. The wondrous hardness and durability of atoms, on which Newton comments, appear to be dependent on a continuance of a temperature within the limited range which permits them to exist, and throughout their career they are controlled by the force of gravitation which, as already shown, suffices to enable nature to effect that purpose. If, however, gravitation thus leaves no room for any action of heat or electricity in directly controlling atomic motion, there is, nevertheless, scope enough for the action of those forces as nature's agents in effecting transmutations between atomic forms.

The drift of the evidence to which I have alluded appears to tend in the direction of showing that changes of temperature cause transmutations of atomic forms, which have, in the laboratory, been

[1] Horsley's edition of Newton's *Works*, iv. 260.

carried so far, from change to change, that at length the material atom escapes detection. In some of those instances, one of the limits of the range of temperature within which that special material substance can exist has perhaps been reached, and, if to be found, it must be searched for in the realms of immaterial force and not among substantial things whilst that same temperature is maintained. Thus, though gravitation determines the affinities between any given atomic forms, there is ample scope for the action of heat in altering the conformation of the atoms, and, by thus necessitating new combinations of them under the action of gravitation, leading to transformations from one substance to another within the limits of temperature which admit of material existence.

With change of temperature in one direction, water changes from its fluid condition and becomes a hard and brittle rock, or with a change of temperature in the opposite direction it becomes a thin invisible vapour. I contend that in those transmutations which nature effects by the action of heat there is no valid reason for supposing that the material atoms remain unchanged throughout the transmutations from ice to water and from water to vapour. Too limited a view seems to be taken of the extent and powers of nature in supposing material atoms to be unchangeable; for that idea in fact places those atoms above the powers of nature, and might indeed almost be said to make them nature's gods. Newton's argument, 'that nature may be lasting, the changes of corporeal things are to be placed only in the various separations and new associations and motions of these permanent particles,'[1] is an expression of that limitation of the bounds of nature. Newton says that 'in the beginning' God made primitive particles 'so very hard as never to wear or break in pieces; no ordinary power being able to divide what God Himself made one in the first creation.'[2] But it surely cannot be considered unreasonable to suppose that the God who uses the powers of nature for controlling those primitive atoms in their career acted also through the agency of powers of nature in their creation, and may also act through the agency of other powers of nature for the purpose of transforming, and even also of dissolving those primitive atoms. In the estimation of material beings matter must necessarily assume a position of special importance; but nature may not perhaps regard it otherwise than as one of numerous forms of force, between which it 'delights in effecting transmutations.' It seems most strange that men of science who admit that the primitive atoms were brought into being by laws of nature should think it unreasonable to entertain the idea that laws of nature may also transform and dissolve those atoms of nature's creation.

[1] Horsley's edition of Newton's *Works*, iv. 260.
[2] *Ibid.*

The evidence of physical phenomena tends, I say, in the direction of showing that the material atoms in ice, water, and vapour are not unchanged during nature's transmutations between those forms of substance; but that, on the contrary, those transformations of substances are effected by transformations of the material atoms of which they are formed. The idea that heat merely drives material atoms farther apart from each other, thus causing transformations of substances without affecting the atoms of which they are composed, is not at all supported by the fact that some fluids contract and others expand in freezing; whereas, if heat acts directly on the atoms and changes their forms, it appears quite natural that in some fluids the changes of form should be such that the action of gravitation, in readjusting them, brings them closer together, and thus causes contraction; whilst in other fluids the atomic forms are changed in such a manner that the readjustment effected by gravitation cannot bring them into so small a space as before, and therefore the frozen substance is expanded beyond the space it required in its fluid form. Water, as already stated, will hold a quantity of salt in solution without increasing in bulk; but the salt thus held in solution impedes the freezing of the water; showing, or at any rate favouring the suggestion, that the atoms of pure water combine under the action of gravitation in such a manner as to leave spaces for the salt to distribute itself throughout the fluid; but, in the act of freezing, those constituent atoms of the water change their forms in such a manner that gravitation can rearrange them more easily without the intervention of salt than with it; and for this reason, fresh water freezes more readily than salt water. The salt water has, in fact, to free itself from the salt, or, in other words, to make itself fresh before the atoms of which it is composed have space in which freely to effect the necessary changes of form; and therefore as the atoms change their forms they endeavour to press the salt away from the spaces in the liquid, unless the application of cold is so great and so rapid as to prevent the separation of the salt from being effected before the water freezes round the atoms of salt, which were merely held in solution in the liquid, and not liable to be affected by the freezing which transforms the liquid atoms. The effort to force away or repel the salt is a natural consequence of the reciprocal action of gravitation being able to effect a closer union, and therefore to exert a stronger force, between the atoms as altered by change of temperature from atoms of water to atoms of ice, than between these latter and the atoms of salt. These comments are, however, offered only as an indication of the manner in which, if it be admitted that heat can effect transformation of material atoms, gravitation affords a sufficient explanation of changes of chemical affinity between them.

It must not be inferred from the foregoing arguments that gravitation or heat can properly be regarded as being itself the actual motive force. On the contrary, I consider that I have definitely shown, in my treatise on the action of vis inertiæ in 'The Ocean,'[1] that gravitation is merely the action by which the vis inertiæ of matter resists motion and restores equilibrium incessantly disturbed by some extraneous motive force, which may, for anything I can say to the contrary, be electricity. For all practical purposes we have to regard gravitation and heat as forces, and in the ordinary use of language must apply the term 'force' to them; but they truly are, nevertheless, merely the forcible actions, or effects, of two opposing forces—one a purely immaterial motive force acting on matter, and the other a power inherent in matter by virtue of which it resists the action of extraneous forces.

The progress of scientific knowledge has made it impossible for us to continue to accept Newton's views as to material substances being transmuted into light without at the same time accepting the idea of natural transitions between matter and immaterial force; and it is merely the crossing of the line between those states of existence, which is necessitated by combining Newton's idea of solid matter being transmuted into light with Hooke's doctrine of light being immaterial force,

[1] Since published under the title *The Ocean: its Tides and Currents, and their Causes*, 2nd edition, Longmans, 1885.

that has formed the difficulty which has prevented the two views from being jointly accepted. But as we can by artificial means transmute the air we breathe into hard metallic substance, it does not seem so very great a step to advance to the idea that in nature's great forge, or crucible within the earth, and in the more stupendous forces of its greater crucible within the sun, the wider transmutations may be effected. I do not here allude to the mere transformation of air at a temperature of 190° C. to a liquid, and at 232° C. to a solid state; in these cases, with a return to normal temperature, the hard substance formed by the freezing of the liquid thaws again to its liquid form and with the further increase of temperature again becomes air, as the boiling of the liquid transforms it to vapour. But besides those transformations there are the well-known phenomena that metals are made heavier by calcination, and that careful experiments in closed vessels have shown that this is due to a transmutation of air into metallic substance during the process of calcination; and in this case a return to normal temperature does not retransform the metal to air. If in an ordinay crucible, in any laboratory, so thin a vapour as air can be transmuted into metal, how much further in the direction of transmutations is it permissible to suppose the furnace of the sun to have effective action?

No chemist can suppose that the fierce central forces of the sun are not effecting transmutations far beyond anything we can ourselves perform in our own tiny crucibles. And if we can easily transmute one of the thinnest and most impalpable of substances into one of the hardest by ordinary artificial means, there does not seem to be much scope for further action in the sun unless in that great crucible nature is not only incessantly transmuting material substance into light, but is also just as incessantly creating matter by transmutation from some pre-existent form of force. This view of the course of nature not only harmonises the otherwise conflicting doctrines of Hooke and Newton, above mentioned, regarding the nature of light, but it also places the question as to the past and future duration of the sun's heat and light on a very different basis from that on which it has been discussed in recent years. The arguments which show that transmutations from material substance to immaterial force result from the resistance of matter to the action of forces within the sun and within the Earth, are based on considerations quite independent of the question as to the maintenance of heat in those bodies; but what those arguments do show is that the creation of heat is a necessary consequence of resistance to the motive forces which cause the sun and the Earth to rotate; and this makes the question as to the duration of heat in either of those bodies to be, not as to how long they require to cool down from temperatures imparted under conditions no longer in operation, but as to whether they will continue to be centres of action in which nature works through material existence from prematerial to postmaterial forces. That such bodies are in fact laboratories in which nature is incessantly effecting transmutations from force to matter and, at the same time, as incessantly transmuting matter into other forms of force, is indicated by the arguments in my treatise 'On the Action of Vis Inertiæ' in 'The Ocean,' which show solar and terrestrial gravitation to be in constant process of creation, and therefore also of transmutation.

The basis on which the question as to the duration of the heat of the sun or of the Earth has recently been discussed is roughly illustrated by the following passage from the work by Lord Kelvin and Professor Tait on 'Natural Philosophy.' They say: 'It is as certain that there is now less volcanic energy in the whole Earth than there was a thousand years ago, as it is that there is less gunpowder in a "Monitor" after she has been seen to discharge shot and shell, whether at a nearly equable rate or not, for five hours without receiving fresh supplies, than there was at the beginning of the action.'[1] That argument is based on the idea that the Earth, under conditions no longer existing, was once heated and is now gradually losing that heat like

[1] *Natural Philosophy*, by Lord Kelvin and P. G. Tait, part ii., new edition, Cambridge, 1895, p. 472.

a mass of molten metal left to cool, or like a furnace left to burn itself out without being replenished with fresh supplies of fuel, so that, though it may flare up from time to time with increased vigour through the collapsing of burning material, it must at length be exhausted. But the other argument to which I have alluded is to the effect that gravitation is the action by which the vis inertiæ of matter resists the motive forces within the sun and Earth, and that the forces of gravitation, heat, and light, are every instant newly created; and as they pass into other forms of force they are re-supplied by the same continuous action, causing a continuous consumption of material substance, which is itself as continuously created by transmutation from other forms of force. The furnace is therefore not left to burn itself out, but is incessantly supplied with fuel; nor is the molten mass left to cool down, but is incessantly supplied with heat created by a conflicting action of universal gravitation on each of its moving particles, causing stress and strain throughout it proportioned to its mass and velocity of axial rotation; and the question is therefore as to the continuity of the transmutation of force into material substance, and re-transmutation of the latter into heat and light.

Lord Kelvin, in his lecture on the sun's heat, delivered on January 21, 1897, at the Royal Institution, says: 'It may be taken as an established result of scientific inquiry that the sun is *not* a burning fire, and *is* merely a white-hot fluid mass cooling, with some little accession of fresh energy, by meteors ocasionally falling in, but of very small account in comparison with the whole energy of heat which he gives out from year to year.' A continuous falling in of the surface, under the action of the sun's gravitation, as it shrinks in cooling, is then supposed to renew almost as much heat in the sun as is radiated from it, so that in this manner the period required for cooling down is immensely extended. This allows the sun to distribute heat around it as long as it can go on shrinking, in process of which it retains all its previous material substance, increased by the few meteors allowed to fall upon it. Thus the sun is supposed to radiate heat into space for millions of years without any change of its material substance. Heat is supposed to have been put into it, under previous conditions, and that stock of heat is supposed to be now gradually escaping from it. And also, in order to avoid the idea that the sun is an active scene in which nature is creating matter from pre-existent force, and through the action of gravitation transmuting matter itself into heat which spreads away in space and supplies warmth in proportion to the force supplied for the creation of the matter from which it is evolved, it is supposed that masses of cold matter rushing through space collide and become a brilliant sun by virtue of the heat caused by their collision, and then again slowly lose that heat and return to their former cold condition, each material atom of the whole being the same unchanged and unchangeable entity throughout its long career. That plausible doctrine, with all the grandeur of its transformations, seems to lead nowhere but back again to its starting-point; whereas the smoother course of nature indicated by the incessant creation of gravitation, to which I have alluded, shows a course by which nature may be incessantly rising on stepping-stones of its past self, never in the aggregate falling back, but continuously advancing to new phases of existence; and for the acceptance of this smoother and wider view of nature's field of action we have to re-adopt Newton's theory of the natural transmutation of matter into light, whilst at the same time retaining Hooke's theory to the effect that light is not a material substance.

# CLERK MAXWELL AND KEPLER'S LAW

When (in my Lecture of December 20, 1877, since published as chapter vii. of the 'New Principles of Natural Philosophy'), commenting on Maxwell's reference to the table on page 115 of his work, 'Matter and Motion,' as showing that a correction dependent on the mass of each planet is necessary to make their relative velocities agree with Kepler's law which declares the squares of their periods to be as the cubes of their distances from the sun, I was misled into a mistake which has perhaps prevented some of my readers from appreciating the more important mistake which I pointed out in Maxwell's explanation. The main point at issue was that Maxwell, together with all reputed authorities at that time, asserted that Kepler's law is rendered inaccurate by a large planet tending to move faster than a smaller one, and he referred to the above-mentioned table as showing that to be the case. I was misled into a misunderstanding of Maxwell's argument because I saw that it is clearly a mistake to suppose that correct theory requires the relative velocities to be dependent on the relative sizes. Knowing Maxwell to be wrong on the main point, I was too hasty with the detail of his argument. The figures of his table to which he refers are :

| | |
|---|---|
| Mercury . . . . . | — 0·0000003 |
| Venus . . . . . | — 0·0000002 |
| Mars . . . . . | — 0·00001 |
| Jupiter . . . . . | + 0·131 |
| Saturn . . . . . | + 0·256 |
| Uranus . . . . . | + 0·37 |
| Neptune . . . . | + 1·6 |

Those figures, with one exception, increase with the distance; but what is wanted for Maxwell's argument is not the figures in that table but the ratio they bear to the relative periods of the planets ; and the figures so obtained would make the four larger planets support his argument. It seems to me that Maxwell ought to have been more explicit in the matter; in which case I should doubtless have immediately given (with the assistance of his table) the correct explanation, which I, in fact, gave in my paper on 'The Secular Acceleration of the Moon's Mean Motion' (chapter xiii. of the 'New Principles of Natural Philosophy') without noticing its bearing on the above dispute. I there pointed out that the sole action of the sun's revolving force would make our year about $365\frac{3}{4}$ days ; but the Earth's force, revolving the sun in about 588 years, reduces the year to about $365\frac{1}{4}$ days. The difference is $365{\cdot}75 \div 588 = 0{\cdot}6$ of a day.

In the same manner the revolving forces of Jupiter and the sun make Jupiter's year 4,332 instead of 4,342 days: a difference of ten days.

Therefore, as it is the sole action of the sun's revolving force which endeavours to make the cubes of the distances as the squares of the periods of revolution, Maxwell's ratio for the latter, as 1 is to 140·701, must be changed to—

as 1·0032 is to 141·3507,
or, as 1 is to 140·891,

which is a closer approximation to the cubes of the distances given by Maxwell as 1 is to 140·832.

The sun's revolving force endeavours to make the planetary periods exactly accord with Kepler's law; but those periods are slightly altered by the similar revolving forces exerted by each planet.

The above correction changes the figures in Maxwell's table from + 0·131 to − 0·059, and, as Maxwell's figures represent the remainders resulting from subtracting the squares of the periods from the cubes of the distances, it makes Jupiter's relative velocity too slow instead of too fast for Kepler's law. The discrepancy is, however, less than half of that shown by Maxwell's table, and it is probably the mean result of two conflicting interplanetary actions. On the one hand, the apparent sidereal revolution of the Earth round the sun is quickened by the joint revolution of the sun and Earth round Jupiter; and, on the other hand, the real motion of the Earth is retarded and that of Jupiter accelerated by the reciprocal action of the two planets on each other. Each of those opposite actions has to be taken into consideration for each planet, and differently dealt with according to their positions as interior or exterior to the Earth's orbit.

## A MISTAKE IN 'THE PRINCIPIA'

In the foregoing essay on the Spinning-Top (page 141), I have given an extract from Newton, alluding to the fact that the reciprocal action of gravitation between small bodies about us is too slight to be perceptible to us. And, in the lecture there alluded to, Professor Poynting suggested that Newton had underestimated that action in consequence of having made an arithmetical error which made 'not less than a month' appear requisite to effect an amount of motion in circumstances under which it would in fact be effected in 320 seconds.

Newton had not the facilities for making test experiments which are now available, and what he properly declared insufficient to fall under the observation of our senses is now made perceptible by the assistance of delicate instruments; but it does not appear to me that he really made the error above indicated, or misjudged the subject in the manner supposed. It is true that the error pointed out by Professor Poynting exists in each of the three editions of Newton to which I have access, but I say the mistake is due to clerical errors in stating the data after Newton had made his calculation correctly from the proper data on which his argument is based.

In Motte's edition, quoted by Professor Poynting, there are two inaccuracies of translation.

*First.*—'Diametro . . . pedis unius descripta' is translated as 'one foot in diameter.'

*Diameter* seems to be a clerical error for *radius*, not only because the phrase 'described with' is more consistent with the idea of radius than diameter, but also because the 'force 20,000,000 times less' than the Earth's accords with radius though not with diameter. Therefore, the term 'diameter' seems to have been erroneously used in writing the sentence after the calculation had been correctly made with the *radius* (not diameter) of one foot.

*Second.*—'In spatiis liberis' is translated as 'even in spaces void of resistance.'

As Newton's argument bears expressly on 'experiments in terrestrial bodies,' it seems out of place to give a calculation on formulæ valid in interplanetary space but not valid in the terrestrial phenomena specially referred to. It seems only reasonable that Newton would, as a matter of course, have made his calculation for the actual conditions under discussion, and that by a clerical error the word 'not' has been omitted from a clause stating the fact of the bodies being *not* 'in free space,' and therefore requiring special allowance to be made for any resisting force not included in those which allow the existing acceleration of gravity about the Earth's surface. The Earth's direct force is certainly in that category, for it is a component of the force acting between the Earth and other bodies, and at the same time a resisting force as regards the action of the two small spheres, hindering instead of assisting their motion towards each other; because, whether the two small spheres be resting on a table, or be suspended by cords, the Earth's gravitation endeavours to keep each in a line directed from it to the Earth's centre; whereas the gravitation of the two small spheres to each other tends to draw them from those lines. Therefore, accepting Professor Poynting's figure of 320 seconds as correct for the conditions of his calculation, an effect of that resisting force as the square root of the surface force of the Earth's gravitation exceeds that of the two-foot sphere (one foot radius) must be added to the time given by Professor Poynting as retarding the motion of each of the two small spheres, thus increasing the time as twice the square root of the resisting force, and making it—

$$320'' \times 8800 = 33 \text{ days},$$

which agrees with Newton's words 'not less than a month.'

The above, I think, shows that clerical errors combined with subsequent mistranslations (which seem to have been consequences of those errors) have changed the data on which Newton based his calculation, so that the conclusion arrived at by him is correct for what appear to have really been his data. It is also correct for what was the object of his argument, whereas a reference to such a space of time as 320″ would make the general tenor of his argument absurd.

# THE ENDS OF THE CORDILLERA DE LOS ANDES

In Honduras there is a line between the Pacific Ocean and the Caribbean Sea along which 'the 'interruption of the Cordilleras is complete . . . 'a great transverse valley reaching from sea to 'sea.'[1]

That crushing of the great meridional range which forms the most striking mountain feature of the American continent in both hemispheres, I have always regarded as a detail of the relative depression of the Earth's surface along the line which divides the bulge of the equatorial regions, due to the Earth's rotation, from the bulge of the northern hemisphere, due to the Earth's onward motion; and, since writing the foregoing essay on 'The Cardioid Earth,' my attention was recalled to that feature of the Central American region whilst listening to Dr. Hans Steffen's lecture at the meeting of the Royal Geographical Society on March 19 this year. In describing his explorations of the Cordillera in Southern Patagonia, his statements as to the disappearance of the great meridional range and the substitution of east and

[1] *Encyclopædia Britannica*, 8th edition (1856), xi. 611.

west ranges as the characteristic feature of the country, seem to me to be in thorough accordance with the idea of a crushing of the great meridional range, similar to that in the northern hemisphere, being a detail of the depression of the Earth's surface which contains the Great Southern Ocean and divides the equatorial bulge due to the Earth's rotation from the Antarctic bulge due to the Earth's onward motion. The crushing of the great Cordillera in the northern hemisphere where it crosses the region indicated as pressed down in the frontispiece of this book was certainly described as above without any knowledge of the reason I have given for it; and I have no doubt that Dr. Steffen's description of the similar crushing of that great range in the southern hemisphere is also independent of any knowledge of the reason I have given for that similar but greater subsidence of the Earth's surface in the southern hemisphere as indicated in the frontispiece. I apply the term 'crushing' to the surface action in the latitudes of the Caribbean Sea and the Great Southern Ocean respectively because, whereas the bulging must tend to create tension along the *crest* of the ridge where traversing the bulge, the subsidence of the surface between that bulge and the bulges north and south of it must tend to create tension at the *base* of the ridge and pressure, giving a crushing, or crumpling, action along the crest of the ridge.

# GLOSSARY

*Inertia.*—An innate property of matter by virtue of which it endeavours to be at rest, and therefore resists motive forces. It is exactly in proportion to the mass of any body. Newton applies the term *vis inertiæ* to this property of matter, and gives it more comprehensive action, as explained in his Definition III., quoted in the foregoing Second Essay, p. 41.

*Vis Inertiæ.*—The force with which matter resists motion. It is as the mass multiplied by the square of the motion resisted. It is always an equal stress in the opposite direction to that of the vis viva of motion. There is no intrinsic impropriety in using this term for *inertia*, as was always done by Newton and generally by myself and many others. In a general sense we may use inertia for vis inertiæ without agreeing with those who wish to exclude the idea of force from the Laws of Motion.

*Momentum* is the mass of any moving body multiplied by its velocity. All are now agreed in this use of this term. I say it is resisted by the inertia of matter in its origin and in its progress, whereas the 'First Law of Motion' (quoted in the foregoing Second Essay, p. 42) supposes inertia to resist its origin but to sustain its progress. It is usually defined as 'quantity of motion;' but that definition, I say, correctly belongs to the square of the velocity. Momentum was at one time used to represent that force, and was then, I say, equivalent to quantity of motion. (See *Vis viva*).

*Vis viva*, or force of motion, is the mass multiplied by the square of the velocity. All are now agreed in this use of this term. It is the effective action of *momentum*. I say there is no difference between the *force* of motion and the *quantity* of motion, so that if vis viva represents the one it also represents the other. We must have a term for mass multiplied by velocity, and another term for mass multiplied by the square of the velocity, and we cannot do better than allow *momentum* and *vis viva* to continue to represent them respectively. As a technical term, momentum appears to have been transferred from square of velocity to simple velocity, on the supposition that the latter properly represents 'quantity of 'motion.' (See *Energy*.) In a general sense *momentum* may be used for *vis viva of momentum.* Vis viva is related to momentum exactly as vis inertiæ is related to inertia.

*Gravity* has gradually become recognised as a term for gravitation less centrifugal force, and represents *weight* and not *mass.* The same mass has more gravity, or weight, near the Poles, where the acceleration of gravity becomes 32·227 ft. per second, than near the Equator, where it becomes only 32·117; whereas in our latitude it approximates closely to 32·2 at the sea level. The above distinction between gravity and gravitation is not always made, and the context must decide as to whether it has been intended by any writer. It has been suggested to use the term *attraction* for gravitation, so as to leave the latter term to represent gravity. But the distinction I have given seems to be the one which is generally accepted; and it seems also undesirable for the term attraction to be appropriated to gravitation.

*Gravitation* is as the mass and inversely as the square of the distance from which it acts. I say: (1) It resists all impressed motion with a force as the square of the velocity; (2) it is the force exerted by matter in resisting motive forces and to restore equilibrium disturbed by expended forces, thus making universal gravitation identical with vis inertiæ or the force with which the inertia of matter resists motion; (3) a rotating sphere exerts a revolving force directly as the gravitation of the nearer exceeds that of the remoter hemisphere, which is inversely as the cube of the distance from the centre of the sphere; (4) in a rotating sphere the radii in the plane of rotation are levers for the resisting action of gravitation; so that if two rigid spheres are of equal mass but one twice the diameter of the other, equal revolving forces will give a velocity of motion twice as great to the small sphere as that given to the large sphere, making the angular velocity four times greater.

*Astral gravitation* is a convenient term, and seems to me the most appropriate possible term, for representing generally the *resisting force* of gravitation opposing all motion, whether it be universal gravitation resisting an impressed force or one aspect of a conflicting action of gravitation. It is the combined action of the gravitation of the universe, *excepting* the force whose action it opposes. Thus, as the Earth carries the ocean along with the Earth's motion, the opposing force of *astral gravitation* is the combined action of all forces of gravitation excepting that of the Earth; whereas the direct action of any motive force, as when a ball is thrown by the hand, is resisted by universal gravitation, which is identical with vis inertiæ, or the force with which matter resists motion. Newton excluded this force by at one time declaring that it destroyed itself by its contrary attractions, and at another time suggesting that we are too far from the centres from which the forces act to enable them to have any effective action on the Earth; but, I say, it has effective action in every detail of terrestrial phenomena.

*Centrifugal Force.*—The definition of *gravity* which I have given above, as being that generally accepted, makes this in fact the difference between gravity and gravitation. In a non-rotating body there is no centrifugal force, and its gravity and gravitation are therefore everywhere the same force. There is also no difference between those forces in the line of the axis of a rotating body. And whatever the difference between them may be along the line of the equator on the surface of a rotating body, it decreases in that plane as the cube of the distance increases. The gravitation of the mass is not altered by its rotation, but continues to be the same force; gravity being its vertical, and centrifugal force its tangential component.

*Evanescence* I have used as a matter-of-fact term for the unknown force which urges matter through its changing phases, and is resisted by gravitation. I at one time thought it not unlikely that electricity might possibly be that force; but Professor Dewar's recent lecture on the work of Sir

Humphry Davy has brought me back to the view I expressed in 1866 as to electrical action being gravitation.

*Energy.*—I have not had occasion to use this as a technical term, but it seems expedient, in face of its widespread use, to show its connection with those I have used. It was at one time applied by Thomas Young to the mass multiplied by the square of the velocity; but for that purpose it is superfluous, as Leibnitz, Newton, and others had already applied the term vis viva to that quantity. It is now, nominally half the vis viva of motion, but is in reality, whenever used in connection with the gravitation measure of work, identical with the quantity to which Leibnitz and Newton applied the term vis viva. In 'gravitation measure' 32·2 poundals has been adopted as the approximate equivalent of gravity in this latitude, because an initial velocity at the rate of 32·2 ft. per second lifts a weight 16·1 ft. in a second, and gravity causes a weight to fall 16·1 ft. in a second with a final velocity at the rate of 32·2 ft. per second. But, as shown in the foregoing essay on the Spinning Top, p. 147, gravity is equivalent to only half the force which gives an initial velocity of 32·2 ft. per second. Forces are as the quantities of motion they cause, and the argument in connection with figs. 32 and 33 shows the initial velocity of 32·2 ft. to give 32·2 ft. of work, or poundals if the weight be one pound; whereas gravity does only 16·1 poundals on the same weight in the second. Instead of 32·2 poundals, only 16·1 should have been adopted as the equivalent of gravity, as 16·1 represents the number of poundals of force requisite to keep a one-pound weight from falling without the further expenditure of force requisite to lift it against gravity. Thomas Young and Newton would arrive at the same results with their forces of 'vis viva' and 'energy,' because they are the same force under different names; and they would also give the same practical results as are now given by one half of that quantity of force, because the present halving of the force merely corrects the erroneous measure of gravitation with which it has recently been connected.

In order to be quite explicit as to the question above at issue, I refer to Article XLVII. in Maxwell's work, 'Matter and Motion,' where he says that a poundal, the unit of force, 'is 'that which, in one second, would communicate to one pound a 'velocity of one foot per second.'

Interpreted by Newton's Second Law of Motion, by which forces are as the quantities of motion they cause, the velocity of one foot per second must be considered to carry the pound weight through the space of one foot; and, as gravity carries it through 16·1 feet in a second, it is therefore, by Newton's law combined with Maxwell's definition, 16·1 poundals; which, I say, is the true equivalent of gravity. But Maxwell *intentionally*, as the context shows, treats his definition exactly as if, instead of 'a velocity of one foot per second,' he had said: a *final* velocity *at the rate* of one foot per second; and he then deals with that quantity, which is the momentum at the end of the second, instead of with the quantity of motion the force has caused; and he thus makes gravity equivalent to 32·2 poundals. That figure represents the momentum of a pound weight after falling for one second, and also the 'acceleration of gravity' per second, the same quantity of momentum being added every second; but momentum is not a direct measure of either the quantity of motion or of the force of motion. It seems to me probable that those who initiated the mistake did so in consequence of having been imbued with the idea that momentum is identical with quantity of motion. Newton's Definition II. of quantity of motion is ambiguous unless considered in connection with the context, which seems to show that by 'velocity' he there means *mean* velocity, or space traversed. At any rate, in commenting on his Third Law of Motion, regarding action and reaction, he expressly warns his readers against dealing with *velocity* of motion instead of *quantity* of motion.

It is indisputable that Thomson and Tait are perfectly correct when they say that 'the *Measure of a Force* is the quantity of motion which it produces in a unit of time.[1] And if momentum, that is to say, mass multiplied by velocity, really represented quantity of motion, it would be quite right to take for a poundal, or unit of force, as they do, the force which, 'acting on one pound of matter for one second, generates a

[1] *The Elements of Natural Philosophy*, Part I., 2nd ed., p. 59.

'velocity of one foot per second.'[1] But as the momentum (which is the velocity at the end of the second) does not represent 'quantity of motion,' it is not a proper measure of force under their own law; and in order to keep in harmony with their own statement as to what is a measure of force they should have made the poundal the force which carries one pound through the space of one foot in one second. That definition really takes quantity of motion for the measure of force, and it makes gravity equal 16·1 poundals instead of 32·2 poundals as declared by them and by Maxwell.

If the unit of force were corrected as above indicated, then *energy* would really (instead of only nominally) represent half the vis viva of momentum, and might become a useful and independent quantity in natural philosophy instead of forming, as at present, merely a complicated method of dealing with vis viva. For, though gravity is equal to only half the initial vis viva of momentum which projects a body to the height of 16·1 feet, and in the second during which the body is rising its resisting action does only half as much work as is done by the force of projection, as soon as the force of projection has been expended by its resisting action it does, in the following second, another equal quantity of work for the restoration of the equilibrium which has been disturbed; making its total action equivalent to that of the vis viva of projection. That second half of the action of gravity is the quantity which may be made available mechanically as stored energy, either by preventing a weight from falling back to its original distance from the Earth's centre until the stored energy is wanted, or by keeping a coiled spring from unbending. I say that the vis viva which has effective action in bending the spring is twice the force exerted by the resistance of the reciprocal gravitation of the particles of the spring (technically called their cohesion), and, in unbending, another equal amount of force is exerted by the gravitation of the particles of the spring for the restoration of the positions which had been disturbed by the force which bent the spring. If a force of projection carry a weight

[1] *Treatise on Natural Philosophy*, vol. i., Part I., new edition, 1879, p. 229.

beyond the distance within which the Earth's gravitation is paramount, none of the opposing force of gravity can be stored, because the force has exceeded the Earth's power of control; and so also, if the bending force break the spring, no force can be stored, because the controlling power of the gravitation of the particles where the breakage occurs has been exceeded by the bending force, which has therefore separated them beyond the distance within which the interaction of their gravitation is paramount.

Another reason for separating a given quantity of vis viva into two parts may be illustrated by the fact that when a weight is lifted one half of the force exerted for the purpose is spent in pushing the Earth away from the weight, and only the remaining half in moving the weight; and *energy* (as half of vis viva) would represent one half, which might be either half, of the force exerted. It would, however, be merely a relative term showing the relation which the force it represents bears to a total quantity of vis viva under consideration. If the force be considered independently of its subordinate relation to the greater force in which it originates, there ceases to be any reason for calling it energy instead of vis viva.

Rankine seems to have introduced the present use of *energy* as a scientific term. In a paper published in 'The Philosophical Magazine' for 1853, page 106, he gives the definitions of *Actual* and *Potential Energy*, which form the basis of his argument, saying: 'If the change whereby potential energy has been 'developed be exactly reversed, then, as the potential energy dis-'appears, the actual energy which had previously disappeared is 'reproduced.' I do not doubt that Rankine correctly shows the relation between the quantities of actual energy and potential energy with which he deals, but he does not recognise the fact that the quantity of actual energy in the first instance must of necessity be at least twice the quantity of the potential energy developed, for (on page 110) he supposes a case 'when the 'tendency to the production of potential energy is simply pro-'portional to the actual energy present.' I say that Rankine ignores not only all the resisting action of gravitation which is a force equivalent to the *vis viva* of the original momentum, but also ignores one half of that momentum; the remaining half is the only quantity of actual energy with which he deals; he shows how it may become represented by an equivalent quantity of potential energy; and how the latter disappears in the reproduction of an exactly equivalent quantity of actual energy. The foregoing argument in connection with the cannon-ball shows that all the momentum of projection is expended by the *resistance* of gravitation; and the 'development' of potential energy depends on the question as to whether the expended force leaves the ball away from the position in which the reciprocal action of gravitation tends to place it. If the force of projection has merely rolled the ball along the ground, there is not any potential energy developed; and the greatest quantity that can be developed in any case is equivalent to only one half of the vis viva given to the cannon-ball by the force of projection. Fig. 33, page 142, shows that the momentum of the cannon-ball does forty-nine miles of work, whatever the direction of projection may be. If projected vertically, only one half of that work is *apparently* effective; but as soon as the original momentum is expended then gravity creates a quantity of effective action for the restoration of equilibrium exactly equivalent to that which it prevented from being apparent in the 90 seconds during which the force of projection was in action. The action is precisely that of the rebound of the bent spring. The force of the rebound is the reciprocal action of gravitation between the particles of the spring, which brings them back to their normal positions *after* the force which bent the spring has ceased to act. And so, also, the reciprocal gravitation between the cannon-ball and the Earth creates the actual energy which brings them back to their normal positions *after* the momentum with which the ball was projected has been expended.

Rankine does not in the paper above quoted, nor in a subsequent paper published in 'The Edinburgh New Philosophical Journal' for July–October, 1855, declare energy to be half vis viva, but seems, on page 136 of the latter paper, to suppose them to be equivalent; though the fact is that his definition of 'velocity,' as 'accelerative force,' being the measure of quantity of work, which is given in the same paper (p. 129), does in fact make his 'actual energy' practically only half vis viva; and in his subsequent article on *Mechanics* in the eighth edition of 'The Encyclopædia Britannica,' published in 1857 (p. 411), he defines *actual energy* of motion as 'one half of the quantity 'which is called *vis viva* in some treatises on Mechanics.' It does not appear that Rankine at first intended to change *energy* from the quantity to which it had been applied by Thomas Young. In every case, however, what Rankine calls the actual energy of the cannon-ball, and is now termed by Lord Kelvin and others its kinetic energy, is intrinsically its vis viva. It is not one half of vis viva, but the full quantity of the identical force which constitutes the vis viva of the cannon-ball's momentum.

Si quid novisti rectius istis,
Candidus imperti; si non, his utere mecum.

# INDEX

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Table I.—The distances east of Japan, in lat 35° N., are given in the top line; and the figures in the columns bel[illegible]m are the temperatures in Fahrenheit degrees. The thermometers used [illegible]pposed to be able to register any temperature intermediate between [illegible]aximum and minimum passed through, and therefore all such records [illegible] as errors. But the distribution of these 'errors' in all parts [illegible]e ocean makes it seem probable that they may generally be considered [illegible] existence of a real rise of temperature, as fully explained in the te[illegible] (See p. 6.)

| Distance | 50 miles | 200 mile | 50 miles | 700 miles | 875 miles | 1,075 miles | [illegible] miles | 1,475 miles |
|---|---|---|---|---|---|---|---|---|
| Surface | 73 | 70·5 | 70·2 | 64·8 | 69·2 | 68·5 | [illegible]1 | 70·5 |
| 10 fathoms | **73·4** | 69·8 | — | 61·5 | — | — | 70·3 | — |
| 20 " | 71·6 | 68·5 | — | 58·2 | — | — | 65·6 | — |
| 25 " | — | — | 68·1 | — | 64·7 | 62·9 | — | 61 |
| 30 " | 70·6 | 67·6 | — | 56 | — | — | 63·8 | — |
| 40 " | 69·7 | 67·2 | — | 54·2 | — | — | — | — |
| 50 " | 67·6 | 66·4 | 64·1 | 52·5 | 62 | 60·8 | 60·4 | 59·2 |
| 60 " | — | 65·1 | — | 50·8 | — | — | — | — |
| 70 " | 65 | 64·8 | — | 50·1 | — | — | — | — |
| 75 " | — | — | 63·4 | — | 61·1 | 58 | 58·9 | 57 |
| 80 " | 64 | 63·3 | — | 48·1 | — | — | — | — |
| 90 " | **64·6** | 62·2 | — | 46·5 | — | — | — | — |
| 100 " | 62·4 | 61·4 | 61·6 | — | 58·9 | 56 | 58·6 | 56 |
| 115 " | — | — | — | 45·2 | — | — | — | — |
| 125 " | 54·2 | 57·[illegible] | 60·5 | 44·9 | 58·1 | 54·5 | 57·2 | 55 |
| 130 " | — | — | – | 42·1 | — | — | — | — |
| 150 " | **64·2** | **57·3** | | **42·3** 40·6 | 56 | 52·2 | 56 | 53·6 |
| 175 " | **55·9**[1] | — | 57 | **41·2** **41·1** | 52·5 | 49·8 | 54·8 | 51·7 |
| 200 " | 52 | 54·4 | 56·6 | 40·3 40·2 | 50·7 | 49·2 | 54·2 | 50·5 |
| 225 " | 50·3 | 50·9 | 54·6 | 40·2 40·1 | 49·2 | 45·7 | 52 | 48 |
| 250 " | 47·7 | 48·7 | 51·4 | 40 40 | 46·4 | 43·9 | 49·2 | 45·9 |
| 275 " | 42 | 47·2 | 49·9 | **40·2** 40 | 43·9 | 42·8 | 47·2 | 43·9 |
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Cold counter-current rising from the north and turning eastwards near the equator.

[1] These figures show a fall of 325 fatho[illegible]les.

[2] These figures show a fall of 600 fathoms in 150 miles.

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